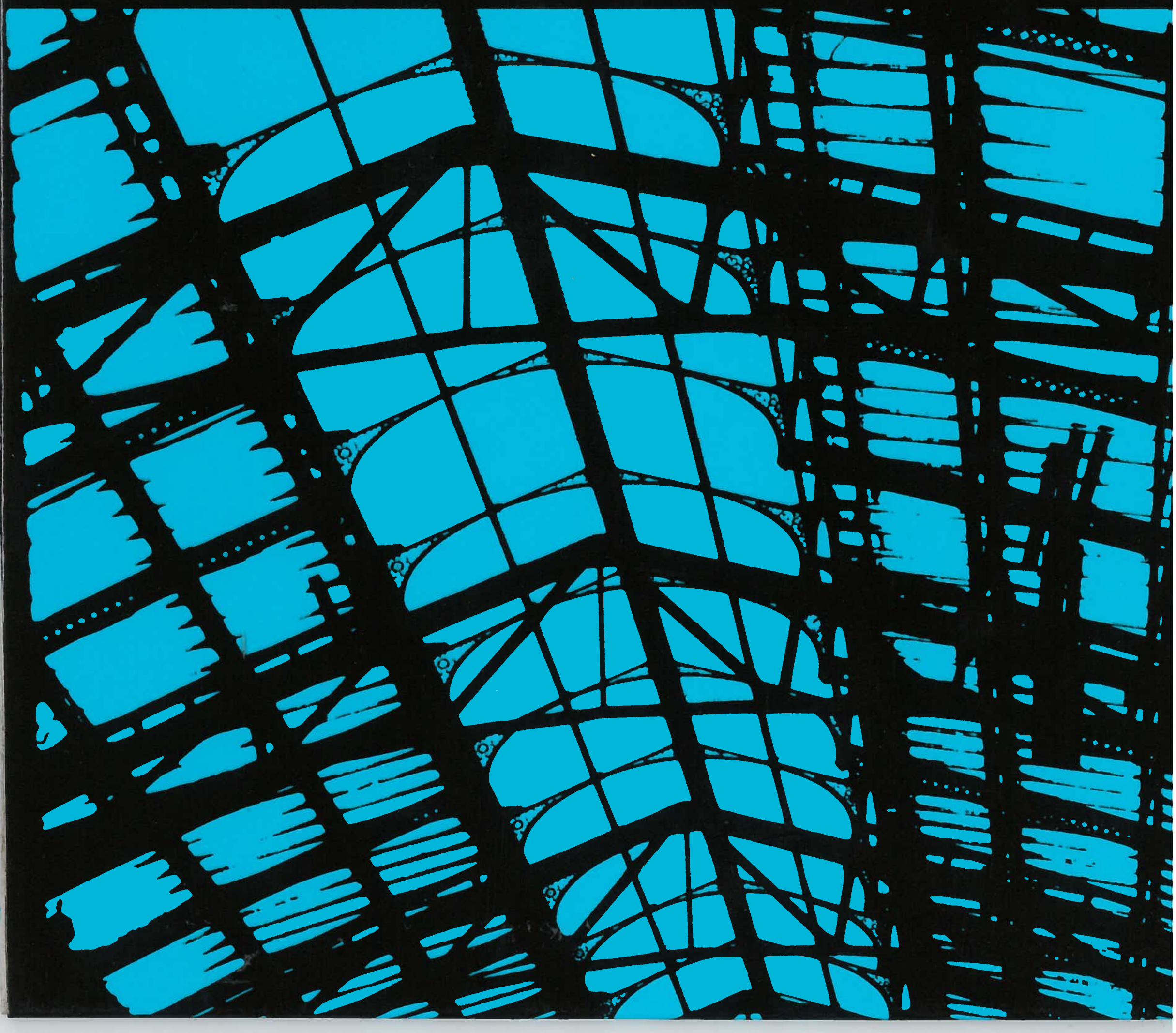
ELLIS HORWOOD SERIES IN CIVIL ENGINEERING

STRUCTURAL ENGINEERING

the nature of theory and design

William Addis



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STRUCTURAL ENGINEERING
The Nature of Theory and Design

WILLIAM ADDIS, Department of Construction Management, University of Reading

Challenging the conventional view that engineering design has its roots in 'theory' and consists in the application of theory in 'practice' this book aims to increase the awareness of the philosophy of engineering amongst practising engineers, engineering scientists and students. The author follows approaches well-established in the history and philosophy of science to give an improved understanding of engineering and design.

The book begins by looking at some problems related to the supposed gap between theory and practice. The author then proposes alternative views concerning the nature of engineering knowledge and the idea of engineering design as a skill. He introduces the concept of the 'design procedure' and uses it in conjunction with Thomas Kuhn's idea of the 'scientific revolution', to look at structural engineering history in terms of design revolutions. These are illustrated by a recent major design revolution — the plastic design of steel-framed structures — and earlier revolutions in Greek and Gothic building design, and the historical development of arch, truss and suspension structures. The author concludes with a number of suggestions for new avenues of research which challenge the contemporary view that engineering science is the only academically respectable area. Finally the author outlines some consequences of his proposed views for the study of the history of engineering and suggests ways in which they could influence engineering education.

Readership: Practising design engineers. Academicians and students in civil, structural and mechanical engineering. Those studying or interested in the philosophy and history of science, technology and design.

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WILLIAM ADDIS M.A., Ph.D.
Department of Construction Management
University of Reading

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On doit encore faire observer qu'il y a beaucoup de choses qui ne peuvent etre connues que par l'expérience.

Les principes de mathématiques et le calcul appliqués d'une manière convenable, peuvent bien faire connaitre la stabilité, l'effort ou la résistance des parties d'un édifice, relativement à leur forme; mais ils ne peuvent pas, seuls, déterminer le degré de stabilité de force ou de résistance qui constitue la solidité de l'ensemble de ces parties, eu égard à leur position, à la manière dont elles sont construites, et au sol sur lequel elles sont établies: car, en faisant abstraction de ces circonstances, on démontrerait qu'un mur isolé et d'à-plomb, pourrait etre élevé à une hauteur indéfinie, quel que fut le rapport de la largeur de sa base avec cette hauteur; c'est-à-dire qu'il pourrait avoir en élevation plus de cent fois son épaisseur, prise par le bas.

Cependant l'expérience prouve que, dans cette position, sa plus grande hauteur ne saurait etre portée à plus de douze ou quinze fois cette épaisseur, et que les murs isolés qu'ont plus d'élévation sont renversés par l'effet de la moindre inégalité de tassement, provenant soit de leur construction ou du sol sur lequel ils sont établis.

Rondelet (1812–7) *Traité de l'art de batir* Vol.5 Avant-propos

It must again be observed that there are many things which can be learned only by experience.

The principles of mathematics and calculation, used appropriately, can give knowledge of the firmness, the load-bearing capacity or the stiffness of the parts of a building according to its design; but alone they cannot determine the degree of firmness, strength or stiffness which constitutes the 'solidité' of the whole, with regard to the disposition of the parts, their manner of construction and the soil upon which they are erected. For, in disregarding these circumstances, it could be shown that a free-standing vertical wall could be built infinitely high, whatever the ratio of the base to the height; that is to say it could have a height more than one hundred times the width of its base.

However, experience tells us that in these circumstances it could be built no higher than twelve or fifteen times the base, and that free standing walls of greater height are toppled by the slightest differential settlement, be it due to their construction or to the soil on which they stand.

PREFACE

I hold the view that if a problem has remained unsolved for two thousand years, it is probably not just because it is a very difficult problem. It is more likely that the problem has appeared insoluble because it has been wrongly conceived. If we look back over two thousand years of designing and constructing architectural and engineering structures, we find there is one such question which has raised its head time and time again:

— how can the gap between 'theory' and 'practice' be bridged?

In this book I have tried to show why this and several related questions cannot be answered directly. In order to break through the deadlock arising from such unanswerable questions, I have considered some other types of question which can be answered and which, hence, can further our understanding—questions such as:

— how did people design architectural structures?
— when people used a body of knowledge they called 'theory', what did they use it for?
— what other bodies of knowledge did people use?

I have also considered the more general question:

— how have people been able to learn from history and avoid the mistakes of their ancestors?

It is an interesting paradox about learning from history that we seem to have learnt some things, but not others. In order to clarify both this matter and the supposed gap between 'theory' and 'practice', I have taken a closer look at the epistemology of

engineering and considered two further questions:

— what sort of knowledge is engineering knowledge?
— how has such knowledge been 'stored' for the benefit of others?

At all times in engineering and architectural history there have been some types of knowledge which have been relatively easy to store and to communicate to other people, for instance by means of diagrams or models, qualitative or quantitative rules, or in mathematical form; this body of knowledge has been growing continuously for at least 2000 years. There are also other types of knowledge which, even today, still appear to be impossible to abstract, condense and pass on to others; they have to be learnt afresh by each young engineer—a feeling for the stiffness of different materials and structures, for instance.

From time to time, throughout history, the un-storable types of knowledge have reached a point of, so to speak, saturation, and a new body of formal knowledge has 'crystallized' out of the saturated solution. After crystallization, such knowledge becomes capable of being stored and communicated to others. It thus becomes more easily taught and learnt and becomes more effective and useful to the engineering design community.

Not surprisingly, in both the history of engineering and the education of young engineers, people have tended to concentrate particularly on that knowledge which is easy to store and communicate. Unfortunately, other types of engineering knowledge have come to receive rather less than their fair share of attention. Even the most obvious means of storing information about existing structural designs—keeping a sketchbook—has now all but died out. Up until about the turn of the present century, it was standard practice for engineers to keep their own notebooks containing annotated sketches of hundreds of interesting designs and details they had seen on their travels; this formed a useful body of knowledge upon which a designer could draw and provided an important link with the past. Also, until the present century, engineering textbooks and encyclopaedias often used to contain many examples of successful designs, both ancient and modern. Nowadays, young engineers are generally brought up without a good knowledge of precedent and to believe that the mathematics of engineering science encapsulates all they need to know.

I believe that we are now coming to the end of a period of engineering history during which the emphasis has been on seeking more and better ways to encapsulate, mathematically, the behaviour of materials and structures—given enough time, money and facilities, this can now be done satisfactorily, even though it may often involve a high degree of complexity. We are now more often faced with the problem of how to achieve adequate results with very limited resources and time. I believe we are now entering an era which will be characterized by the search for more and better ways of encapsulating, storing and communicating the knowledge which engineering designers have about how they design, and how they marshall their skills to deal with a superfluity of information and lack of time.

My evidence for these beliefs is that the last decade has seen three important developments concerning structural design methods: the first is a subtle change in the perceived function of the many Codes of Practice which serve the design professions in the construction industry; the second is a growing interest in so-called 'expert

systems';† the third is a gradually increasing realization that too little of an engineer's education is nowadays devoted to the art of structural design, while too much time is devoted to the computational tools of the trade (imagine the outcry were an artist's education devoted entirely to the chemistry of mixing paints).

It might, then, be argued that this book is particularly timely. Indeed, I very much hope that the ideas I have presented do help to change attitudes towards the nature of engineering design and to encourage the study of design and its history as part of an engineer's academic education. It was not, however, with such an aim in mind that the book came to be written.

My book had its origins in the context of the history of buildings. A friend's enthusiasm for the history of architecture had inspired in me an interest in the nature of the engineering input to the design of great works of architecture such as Greek temples, Gothic and Renaissance cathedrals and the magnificent railway stations of the 19th century.

My attempts at satisfying this interest soon yielded much history of architecture and quite a lot of history of technology; but it yielded very little on the history of engineering design. I found no satisfactory answers to my apparently simple question, 'How did engineers (or whatever they were then called) design the load-bearing elements of buildings and bridges?'. I also found that, while a lot had been written on why certain architects and buildings are now considered to be great from an architectural standpoint, there was hardly any corresponding discussion of the quality of obvious masterpieces of engineering design. (I later found that there is, in fact, a little more than I first believed, but mostly in books long since out of print and in specialist periodicals.)

I also discovered a rather patronizing attitude to many engineering achievements of the past—surprise was often expressed that great engineering structures could have been designed without a knowledge of modern statics, elasticity and theory of strength of materials; their design was held to be the result of a rather haphazard development called 'trial and error' and the use of 'mere' empirical rules. I have even encountered the suggestion that craftsmen and eminent engineers of the past might not have been aware that a beam loaded in bending suffers compression in one surface and tension in the other (presumably in the mistaken belief that Galileo had been the first to 'discover' this fact in 1638). Even the very few mentions of early engineering design methods which I did discover tended to emphasize the differences between them and modern 'scientific' design; any similarities were neither sought nor found.

My early searching, then, yielded few answers to my questions about the history of engineering design. I had to change my approach, not only in order to answer my original questions, but also to investigate why they seemed not to have been answered already. It was this change of emphasis which led to the need to try to understand the very nature of design in civil and structural engineering, not only in the past but also in the present.

†My own feeling about expert systems is that, at present, they are mistakenly being used to try to store knowledge with a view to being able to replace people—the latest version of man's ancient quest to produce the perfect automaton. A more fruitful quest would be to develop tools which would better serve the needs of practising designers without any threat of de-skilling them.

I began by trying to establish just why it was that the great achievements of structural engineering and of designers such as Brunelleschi, Wren, Eiffel, Maillart, Nervi and Torroja, were great. In pursuing this end, however, I got rather side-tracked by what such people had written about structural design. I found that they (and many others) said very different things from what I had been led to believe by much that I had read on the history of architectural engineering. I was, incidentally, also side-tracked by the sheer pleasure of coming to understand just what it was that some of the greatest men in history have contributed to our present world—by studying the structures and buildings they designed and by reading, in their own words, their beliefs about what they were doing and about the world in which they lived, and how they came to understand, for the first time, matters which today are too easily taken for granted. In addition, there was the enjoyment I found in looking at the thousands of old books (especially at the beautiful illustrations they usually contain), which make up the patchwork which is the history of architectural engineering.

What followed was a journey through many writings, from many ages, on many subjects. I have tried to present an account of this journey, together with some of the ideas I had while travelling, and weave them together into a subject which I call the Philosophy of Engineering. While this may save others the need to tread the particular path which I blazed, I discovered during my journey a great many more paths which need exploring. And there still remains, of course, one of my original goals, that of establishing 'an aesthetics of structure' and 'structural criticism' which would help in discussing and evaluating the quality of works of structural engineering in a manner similar to aesthetics and criticism in the fields of architecture and music—Brunelleschi's dome over the cathedral of Santa Maria del Fiore in Florence, for instance, is as much of an engineering masterpiece as Le Corbusier's church at Ronchamp is an architectural masterpiece and Mozart's Requiem is a musical one.

For the enjoyment I have had during many hours of discussing the history and philosophy of structural engineering, I am grateful to a large number of people who share my enthusiasms, many of whom have now become friends—I mention Frank Newby and James Sutherland in particular. I am also especially indebted to Dana Kubick, who first inspired me to develop an interest in architecture and in history, and to Tess Nowell who has been both patient and supportive during the months I have spent sitting at a keyboard producing this book.

London W. Addis
July 1990

1

Concerning the nature of design

1.1 INTRODUCTION

The word 'design' has become something of a Humpty Dumpty word meaning just about anything the user wants it to mean. Early uses of the word, both as a verb and a noun related to an intention to do something. Later it included the production of a drawing. The Oxford Dictionary gives the following meanings and dates of early recorded use

1548	to plan or plan out
1570	to trace the outline of; to delineate
1588	purpose, aim, intention
1593	a plan or scheme conceived in the mind of something to be done; the preliminary conception of an idea that is to be carried into effect by action; a project
1638	a preliminary sketch for a work of art; the plan of a building after which the structure or texture is to be completed
1644	the combination of details which go to make up a work of art
1655	to purpose or intend
1657	the thing aimed at
1662	to draw, sketch
1666	to plan and execute;† to fashion with artistic skill
1668	to mark out, to indicate (last use)
1697	to make the preliminary sketch of; to make the plans and drawings necessary for the construction of

The Dictionary has yet to catch up with more modern usage — design as a verb now relates to almost any part of the act of creation of almost any product, from a building

† This meaning is illustrated with the following citation from 1865, 'the Roman bridges were designed on the same grand scale as their aqueducts'.

to a T-shirt or hair style, while, as a noun, the word now often refers to an abstract quality of an artefact, something about the way it looks, its image, its style.

Curiously none of these meanings includes what engineering designers would say is its principal meaning—to describe what they are doing when they design a bridge or building or aeroplane or car.

Even within this specialized sense of the word, which is the concern of this book, there can still be confusion or ambiguity. If one were to ask who designed the Sydney Opera House, some people might reply Utzon (an architect) while others might reply Ove Arup and Partners (a firm of consulting engineers). This ambiguity is even more common in the past when professional responsibilities were different. There is frequently also ambiguity about which stage of a project, or level of detail is implied—scheme design or detail design. And last, but not least, is the ambiguity about some of the mathematical work, often called 'structural analysis' by structural engineers, which might be undertaken during design.†

In broad terms, then, I will be focusing on the sense of the word design which relates to its use, as a verb, to denote the creative activities associated with civil and structural engineering and, as a noun, to the immediate products of this creativity—usually drawings and lists of components. When looking at the present I shall not be dealing with purely architectural aspects of design though; as we go into the past, this clear distinction quickly becomes blurred, as we shall see.

As well as eluding the compilers of dictionaries, the word 'design' is a curiously English idea. It cannot be translated into French, German or Spanish and, conversely, the words used in these languages to describe the activity of engineers do not encapsulate the whole of what English speakers would want to say—we are indeed prisoners of the language we speak. But, fortunately, it is far easier to use words than it is to define them. For much of the time, context and agreed usage smooth over the ambiguities and disagreements. It is therefore quite likely that most readers would understand what the following sentences meant without needing to agree on a precise definition:

- Cathedrals were designed without the use of statics
- Early suspension bridges were designed empirically
- Sir Benjamin Baker designed the Forth Bridge
- The Code of Practice, BS 8110, relates to the designing of buildings made of reinforced concrete.

But what happens when we do delve deeper? In fact, we come across some rather significant differences of opinion and arguments which are not able to be resolved amicably. These could perhaps be overlooked if they were merely matters of taste, such as views about the importance of Haydn as a composer. It is my belief, however, that the differences are more significant and have a profound effect on the practising of engineering design, particularly as a consequence of the type of engineering education which has come to be accepted, at least in Britain.

In looking at design in structural engineering, it is not my intention to try to provide a precise, succinct definition which encapsulates the engineer's sense of the word

† It has often puzzled me why there are no books called 'mechanical', 'electronic' or 'thermodynamic analysis' for engineers in other fields.

'design'. Nor, indeed, is it to set down precise instructions as to how to design. Rather it is to discuss what designing is, and what it is not, and what it has been in the past, in order to come to understand it better. Design is seen by different people in different ways and several of these will be encountered later. Probably the most prevalent way in which research scientists, practising engineers and educators try to encapsulate the meaning of design is by means of several phrases which include the words theory and practice. It is to this view which we now turn.

1.2 'THEORY' AND 'PRACTICE'

The words 'theory' and 'practice' are deeply embedded in our culture. Both words are extremely old, although their meaning has changed from age to age and language to language. Their use is not, of course, exclusive to civil and structural engineering; it is ubiquitous—in architecture, most other branches of engineering and, indeed, in many other disciplines as diverse as music, painting and sailing. And in each case the activity itself is often described by phrases such as 'putting theory into practice" or as 'applying the theory of a subject'. Even more frequent than the use of the words theory and practice in print, is the use they have acquired in everyday conversation, by engineers and non-engineers alike. Examples such as the following are common:

in theory it should . . . .
theoretically it ought to be, but . . . .
that's all right in theory, but in practice . . . .

A dictionary also reflects this usage:

theory	that department of an art or technical subject which consists in the knowledge or statement of the facts on which it depends, . . . as distinct from its practice.
practice	action as distinct from . . . theory, knowledge, etc. [SOED]

Their use in the field of engineering is nicely encapsulated in the following phrase, attributed to an unnamed French writer, which appeared in an article entitled 'On the Principles and Practice of Civil Engineering' written in 1858 by the engineer Henry Conybeare. It also expresses very clearly the generally supposed relationship between the theory and practice.

> La théorie c'est le général; la pratique ce sont les soldats. [Conybeare 1858:39]

This article was written in the middle of the century which saw what is usually regarded as a transition period in engineering. At the beginning of the 19th century 'rules of thumb' were supposed to be the principal means of determining the dimensions and proportions of buildings. By the end of the century this task usually involved calculations using the mathematics of statics, elasticity and strength of materials. From the mid-19th century to the present day there has been a continuing stream of writers who classify the constituent activities of civil and structural engineering as being either 'theory' or 'practice' and suggest that it is the latter which

is utterly subservient to the former. From the earliest days of technical journalism comes the following example:

> In strictness, theoretical knowledge ought not only to be the foundation of all practice, but it ought to be regarded as such, and acquired as such by the practical man. [Anon 1842:2]

Throughout the history of the Institution of Civil Engineers, the idea that practical men should be educated in theoretical matters and should be seeking to apply this knowledge in their work has been a favourite theme for Presidential Addresses. George Bidder, for instance, chose to define 'the object and scope of the profession of the Civil Engineer' as:

> to take up the results discovered by the abstract mathematician, the chemist, and the geologist and to apply them practically for the commercial advantage of the world at large. [Bidder 1859/60:218]

In 1893 a special lecture series was started at the Institution of Civil Engineers, the James Forrest lectures. Their theme was, and still is, The Interdependence of Abstract Science and Engineering. However, despite the title, and with only one or two exceptions, the relationship is portrayed, rather, as the Dependence of Engineering upon Abstract Science. This trend followed the very first James Forrest lecture where theory is lauded, despite mention of its misuse:

> Since 1848 the supremacy of theory over rule of thumb has gradually but surely asserted itself, though, at times, the want of common sense and experience in the application of abstract principles, as well, perhaps, as ill-judged efforts to produce cheap structures, has led to disasters quite as serious as those which arose from want of theoretical knowledge. [Anderson 1893:267]

Anderson continued by hailing, as the greatest triumph of theory, the Forth Bridge:

> for the design and construction of which no tentative experiments were needed, though the form and mode of construction were very special, if not absolutely new; and the dimensions, both in span and height, so gigantic that the authors of the design could have derived but little aid from previous experience.† [Anderson 1893:267]

Similar attitudes are betrayed by a great many of the other James Forrest lecturers, for instance:

> The mistake caused by the wrong application of mathematical formulas is only to be cured by a more abundant supply of more powerful mathematics. [Hopkinson 1894:345]

> [A James Forrest lecturer] has to show how the visions of the philosopher become translated into the matter of fact of the engineer. [Ewing 1899:289]

> [The profession of civil engineering] is essentially concerned with the application of scientific principles to the works of construction. [Pippard 1961:129]

†For the designer's different view, see p.12.

Pippard had, in fact, already expounded his views on the nature of engineering more fully in the Centenary Number of *The Engineer* and it is worth repeating some of them here because of the weight they carry coming from the pen of such a well-known name:

> It is largely due to the efforts of . . . the minority who devoted themselves to the theoretical aspects of the professional work . . . in the past that the practical engineer has succeeded in gaining and, more important, maintaining the professional status which . . . he values so highly. The theory of today should be the practice of tomorrow and unless theoretical knowledge is ever in advance of current practical requirements the survival of the engineer as a professional man is in danger . . . The practitioners [must be] educated to understand and translate into reality the work of the scientist. [Pippard 1956:161]

These several citations illustrate the commonly accepted use of the terms 'theory' and 'practice' in discussing civil and structural engineering. They could be augmented by hundreds of other examples, for the two terms have been in regular use in similar contexts since the earliest days of the printed book right up to the present day. During the early days of engineering periodicals (1830–1860), their pages are littered with exhortations for 'practical men' to use theory to their (and society's) advantage. In reply, there was also, however, a significant number of correspondents riposting that 'highfalutin' mathematics' (as one of them called it) was of no use at all to 'practical men'.

Underlying the many exhortations encouraging the use of theory is the assumption that engineering practice is the application of theory. The mood of that period, which is frequently suggested to be the dawn of the first 'application of theory' (roughly between 1825 and 1875), is captured by one of the greatest engineers of the day, William Fairbairn, in a lecture delivered in 1852:

> I sincerely believe that there is yet before us a coming age when the exact rules of physical truth will be brought to bear upon the constructive and useful arts, with the same certainty and effect in the practical operations of the artificer and the mechanic, as they now do in the laboratory of the chemist or the observatory of the astronomer. [Fairbairn 1856:100]

Other writers were even bolder in their claims for the supremacy of theory; a leader in 'The Practical Mechanic and Engineer's Magazine' of October 1842, entitled 'Theory and Practice', asserted that:

> while practice indicates a certain mode of action, theory points out the most correct way. And, if this be correct, our next conclusion is, that practice, unaided by theory, has only a chance of being right; whereas theory can never be wrong; and that practice, which is not entirely in accordance with theory, must be erroneous. [Anon 1842:1]

1.3 THE GAP BETWEEN THEORY AND PRACTICE

The theory/practice classification is not only popular, it is undoubtedly very useful. It can be a relatively easy task to browse the relevant shelves of a library and slot books into the one category or the other: works devoted to mathematics, statics,

strength of materials, the theory of buckling or of shells and the many books on various techniques of structural analysis clearly belong on the theory side. Books covering techniques of construction, site management, the welding of steel frame structures, the placement of reinforcement in concrete, the workability of materials, techniques of joining structural elements, and so on, belong to the practice side.

However, there are some books which seem to resist classification according to theory/practice categories, yet which seem nonetheless central to the activity of engineering. These books treat of the bridging activity which attempts to relate or to join theory and practice. An example of such a book is *The Philosophy of Structures* by Eduardo Torroja [Torroja 1967]. It is one of the few books which deals directly with design and the way a design engineer uses knowledge such as an understanding of the behaviour of materials and structures. This apparently small problem of taxonomy might be overlooked if it were an insignificant and isolated difficulty. However, as will become clear, it is only one manifestation of a more profound issue which affects the whole way we view the activities of civil and structural engineering, their history and their teaching.

Although the classification of engineering into theory or practice can be easy, it can also lead to confusion—about what the terms actually mean and even between 'theory' and 'a theory'. 'Theory' can variously be used to include any or all of the following:

- bodies of mathematical knowledge
- mathematical theory of, for instance, elasticity
- theories of strength of materials
- structural analysis
- mathematical modelling of structures, loads and materials
- empirical data and rules
- empirically derived constants used in design calculations.

Similarly, the word 'practice' may refer to any or all of:

- construction of buildings
- design of buildings or engineering works
- laboratory experimentation
- testing of actual whole- or part-structures
- establishment of empirical data and rules.

One method of clarifying the above confusion might be to try to set down precise criteria for deciding where theory stops and practice starts. As will become clear in later discussion, such a method is likely not to be fruitful and the solution adopted will be to propose an alternative classification (see §4.3).

A further difficulty with the theory/practice classification is that the relationship between the two areas is unclear. Several of the citations given above suggest that practice entirely depends upon theory. A similar idea is expressed by phrases such as 'applying theory in practice' and 'converting theory into practice'. There is, however, an even more profound difficulty. Whichever meaning of the two terms be adopted, and whatever be the supposed relation between them, there nevertheless has been,

and still is, a widespread perception that there is a gap between them and that it is someone's task to the bridge it. As recently as 1982 a contribution to a Symposium entitled 'Collapse—the Buckling of Structures in Theory and Practice' referred to 'a promising means of narrowing the present chasm between theory and practice' [Thompson & Hunt 1983:356].

Unfortunately, it has seldom been made entirely clear just whose job it is to bridge the gap. Over the last 200 years there has developed an image of scientists remote in their ivory towers and an image of practising engineers as conservative and reluctant to take up new knowledge.† As has been illustrated, the 19th century was full of exhortations for 'practical men' to take up the work of the scientists, but rather little help in showing them how to do it. There had, in fact, been another period when practical men were being told to make use of theory, namely, the 12th century (see §12.3). It is interesting to note that two periods during which there were especially dramatic developments in structural engineering (the Gothic and Victorian periods) are precisely those during which the cries to bridge the gap between theory and practice were loudest—perhaps this fact is more than a mere coincidence.

In addition to the problem of there being a gap to bridge between theory and practice, there is a further area of confusion. This concerns the questions as to 'which theory?' and 'which practice?' and hence 'which gap?'. There are two entirely separate sets of surroundings in which these notions are current. One is the teaching and research environments of the polytechnics and universities; the other is the design offices of engineering firms. In these two sets of surroundings, the notions of theory and practice are used in very different ways.

In the university environment theory and practice are associated with the laboratory experiments and tests which are the concern of engineering scientists. The 'practice' serves both to gather raw engineering data, and in some way to check the 'theory'. It is often reported that 'a close correlation between theory and practice has been achieved'. This is counted as a mark of success and is often accompanied by the recommendation (often not merely implied) that the engineering design profession ought immediately to incorporate that scientist's results into their working routine.

In the design office the word 'theory' might well apply to a theory of elastic bending which has been meticulously checked and 'verified' in sophisticated and extremely accurate laboratory experiments—the direct product of a scientist's work. It might also, however apply to the use of an approximate rule or even the use of an equation of statics or strength of materials in an entirely 'wrong' context. It may be known, for example, that a beam in a structure is not pin-jointed at its ends, and yet the maximum bending moment it has to sustain might be calculated to be that appropriate to a simply supported beam, namely, $WL/4$ (load × length / 4). Alternatively, although known not to have fixed ends it may be designed using calculations appropriate to a beam with fully restrained ends, $WL/8$ (to take account, perhaps, of some extra stiffness in the floor). Both of the above cases might surprise the scientist a little in the use of assumptions known to be inaccurate. However, his surprise would become alarm upon observing the use of the formula $WL/6$ for the case of a beam believed to be 'partially restrained'. Unlike the previous two formulae, this last one

†See [Cross 1952] and [Fores & Sorge 1978] who discuss the scientist's 'marketing problem'.

has no theoretical justification at all. And yet a designer might be quite happy with such approaches—after all, their use can yield safe and economic structures!

The above examples illustrate two very different attitudes to theory and practice. The results of laboratory work may only have application in the highly controlled 'practical' environment of the research laboratory and not in the very different circumstances of the 'practice' in the design office and on the constuction site. Furthermore, a scientist's proposals may involve a degree of sophistication and complexity entirely out of proportion to the scale of the designer's practical problem they would solve. The gap between the knowledge embodied in theory and science, and how that knowledge can be used, is again manifest.

This confusion can be summarized in the ambiguity as to which gap is meant by the generally perceived 'gap between theory and practice'—it is often unclear which of the six possibilities shown in Fig. 1.1 is actually meant.

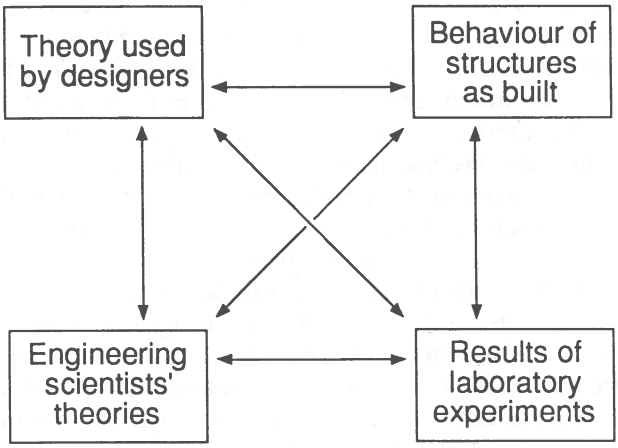


Fig. 1.1—Several possible gaps between theory and practice.

Some examples typical of the confusion between the different attitudes and aims of the engineering scientist and the designer can be found in the report of a 'Conference on the Correlation between Calculated and Observed Stresses and Displacements in Structures' [Report 1955]. Significantly, the report contains no specific brief as to the aims of the conference. It is thereby tacitly assumed that the aim of both scientists and designers is the establishment of the actual stresses and displacements which are to be found in real structures. The result is the jumbled presentation of many different issues, some relating clearly to engineering science, some relating rather less clearly to the subject in general and a few relating almost obliquely to design. The following list illustrates the wide range of topics covered in the conference and the almost total lack of structure and attempt to relate the experimental work to the requirements of the designer:

- methods of design;
- theoretical techniques used in design;
- mathematical models of real structures, loads and materials;

- the assumptions made in creating these models;
- the use of mathematical models in design, in analysing proposed structures and in analysing existing structures;
- the relation between mathematical models, real (scale) models and actual structures;
- the boundary conditions relevant to models and to actual structures;
- the techniques of testing actual structures, especially the manner of applying loads;
- the relation between loading patterns used in the experimental investigations and those to which the actual structures would be subjected in service;
- the relationship between laboratory experimentation and tests on actual structures;
- the significance of the results of both of these for the engineering designer;
- the manner of measuring stresses and displacements and the meaning or interpretation of the measurements;
- the relation between strain gauge readings and actual stresses (whatever they may be);
- theories of engineering science;
- the values of empirical constants used to modify the results of using mathematical models and theories of engineering science;
- the understanding of how real structures actually behave under load.

Most seriously of all, there is no mention or discussion of the criteria by which the various investigations and their results were, or should be, judged and from whose point of view they should be appraised. Also entirely overlooked was the fact that, in any case, it is literally meaningless to talk of a comparison of theoretical predictions with actual stresses in a building when no measurements are likely to be taken. This is, in fact, the case for most buildings which are designed and constructed.

In summary, conventional views of structural engineering, making use of the terms theory and practice, fail to distinguish adequately between two distinct and very different types of activity and purpose—those of the engineering scientist and of the engineering designer.

1.4 THE ROLE OF THEORY IN DESIGN

Having mentioned the different goals of engineering design and engineering science, it should also be mentioned that there is more than one view of design which are distinguished by their different attitudes to the role of theory in design. One view, already illustrated, is that engineering design depends utterly upon theory of some kind. This issue was discussed by Professor Sir Alan Harris as one of the 'Architectural Misconceptions of Engineering' [Harris 1961] but it is a misconception of many non-architects too, including a few engineers. This attitude towards structural design leads to the unconcious and almost inevitable conclusion that design is, ideally, a scientific process, a type of applied science. Thus structural design is seen as a direct application of mathematical structural analysis and its ultimate aim is to predict exactly the supposed stresses and strains in a structure under load. These persons are

the modern adherents of the view, expressed by Fairbairn in 1856, that 'there is yet before us a coming age'† when the engineer's calculations will be as precise as those of the astronomer.

Progress, as perceived by the holders of this view, is deemed to be the development of new and ever more sophisticated techniques of structural analysis, and the ever more precise determination of the supposed actual stresses and deflexions in real buildings. The Factor of Safety is seen rather as a Factor of Uncertainty, and as having a historical trend towards unity. Indeed, writing about his design of a bridge in 1878, the eminent American engineer, Theodore Cooper, hailed that Fairbairn's 'coming age' had arrived:

> It was the first paper on bridge construction in which that relic of ignorance, *the factor of safety*, was entirely omitted. [Cooper 1889:24]

Cooper also commented that Benjamin Baker's Forth Railway Bridge was:

> the clumsiest structure ever designed by man; the most awkward piece of engineering in my opinion that was ever constructed. An American would have taken that bridge with the amount of money appropriated and would have turned back 50% to the owners instead of collecting, when the bridge was done, nearly 40% in excess of the estimate.‡ (cited in [Sibly & Walker 1977:199])

There is, however, a second, entirely different view of the role of theory in design. Its advocates consider theoretical calculations to be of secondary importance to a different type of knowledge. This other type of knowledge is a qualitative one, based upon an understanding of how materials and structures behave, rather than upon the abstract principles, laws or theories which are supposed to govern their behaviour. The names of Torroja, Nervi, Candela and Maillart are often associated with such views:

> The calculation of stresses can only serve to check and to correct the sizes of the structural members as conceived and proposed by the intuition of the designer. [Torroja 1967:331]

> The most advanced chapters of theory of structures . . . can only be used to check the stability of a structure. They can be used only to analyze numerically a structure already designed, not only in its general outline, but in all its dimensional relations. The formative stage of a design, during which its main characteristics are defined and its qualities and faults are determined once and for all (just as the characteristics of an organism are clearly defined in the embryo), cannot make use of structural theory and must resort to intuition and schematic simplifications. [Nervi 1956:17]

> As our reward for inventing a *Theory of Structures* we are now obliged to calculate everything we build and to justify it with numbers. This can be really discouraging to the adventurous mind. If we wish to surpass the pedantic pace of the building codes we must defend our audacity with a formidable array of figures and equations.
>
> Although the extent to which we can assess numerically the real behaviour of a structure is rather hypothetical, mathematical calculations have a certain value. They can

†For the full citation, see p.5.
‡His own Quebec bridge collapsed during construction in 1907.

> give us a rough assurance that the structures will stand after we have called on our experience and common-sense to determine its form and dimensions. [Candela c.1950]

> It is admittedly a fairly widespread opinion that the dimensions [of a structure] should be unequivocally and finally determined by calculation. However in view of the impossibility of taking into account all possible contingencies, any calculation can be nothing but a guidance to the designer. [Maillart, cited in Straub 1952:240]

This brings us to yet another problem with the view that design is a matter of 'putting theory into practice'. It excludes the possibility that design (or 'practice') could be dependent upon types of knowledge not included in the category called 'theory'. Yet every practising engineer, not only the four eminent ones cited above, knows that design depends upon and is influenced by many other types of knowledge. Some examples are:

- rules of thumb
- the numerous empirical data and rules associated with Codes of Practice
- the properties of particular materials
- factors of safety
- intuitive knowledge of structural behaviour
- experience
- engineering judgement

Such knowledge does not easily fall under the heading 'theory' yet neither does it really belong to 'practice'. It appears to be caught in the same 'excluded middle' as design and, like design, is thereby often rendered invisible.

1.5 DESIGN—SCIENCE OR ART?

As with other words, finding a precise definition does not necessarily help the discussion. The words 'science' and 'art' have changed in meaning more than most over the last five hundred years and yet Aristotle's discussion has stood the test of time (and translation) very well:

> Scientific knowledge is of things that are never other than they are . . . All scientific knowledge can be imparted by teaching and what is known in this way can be learned . . .
>
> An art [is] a rational faculty exercised in making something. In fact there is no art which cannot be so described, nor is there any faculty of the kind that is not an art. It follows that an art is nothing more or less than a productive quality exercised in combination with true reason. The business of every art is to bring something into existence, and the practice of an art involves the study of how to bring into existence something which is capable of having such an existence and has its efficient cause in the maker and not in itself. [Aristotle (c.350 BC) 1955:174–175]

These definitions help to clarify the epistemology of the subjects. Scientific knowledge, in Aristotle's sense, contains no hint of explanation, while his idea of art does imply the use of reason. These points will later be seen to be important (see also [Layton 1974]).

Sadly the modern use of the word 'art' often misses its older sense and implies only painting, pottery and the like. There are fortunately still exceptions, for instance *Zen and the Art of Motorcycle Maintenance* [Pirsig 1974] and 'Civil Engineering considered as an Art' [Harris 1975]. 'Science' on the other hand has changed its meaning considerably. It now does include the idea of explanation and understanding. In Aristotle's day (and up to about 1830) science was simply the raw data;† it could then be interpreted using philosophy. The word 'science', and especially the adjective 'scientific', also now contain a note of approval and recommendation. 'Scientific' design is meant somehow to be better that 'ordinary' design!

The discussion earlier in this chapter will have left the reader in little doubt that I believe structural engineering design to be an art—an art which draws upon many skills and bodies of knowledge in its execution. I am not alone.‡ The answer to the question posed in the title of the present section seems to be so clear that another question raises itself, namely 'how is it that anybody could believe otherwise?'. The answer to this question is more complex and will take a further 200 pages or so to answer. Briefly, it is embedded in our education system, our picture of history and our being brainwashed to believe that 'scientific' is virtually a synonym for 'infallible'.

The magnitude of the deception at hand can hardly be better illustrated than by comparing the words of one of the James Forrest lecturers with those of the designer whose work he was discussing. It will be recalled from §1.2 that, in the first James Forrest lecturer on the Interdependence of Abstract Science and Engineering, in 1893, claimed that the design of the Forth Bridge was the greatest triumph of theory and that it 'could have derived but little aid from previous experience' and had required 'no tentative experiments'. These sentiments could hardly be more at variance with those of the bridge's designer, Benjamin Baker. In his paper to the British Association, Baker carefully documented the many investigations he had made in order to dispel as much doubt as possible about all aspects of the design. He had studied all the tests made on rivetted plate structures in the previous 50 years; he was entirely familiar with all the experimental work which had been done in developing Stephenson's Britannia Bridge in the late 1840s; he had studied previous testing and the use of tubular structures; he carried out extensive tests comparing the use of wrought iron with the new material, steel; he carried out many tests on rivetted joints and upon the performance of rivetted structural members in compression; he investigated the raising of steel's yield point by work hardening; he considered the effect of differential heating of parts of the structure; and, especially in the light of the recent Tay Bridge disaster, he had thoroughly examined the loads exerted by high winds. He concluded:

> It will be gathered from the preceding necessarily brief and incomplete description of the proposed Forth Bridge that no novel or untried elements enter into the design . . . The merit of the design, if any, will be found not in the novelty of the pinciples underlying it, but in the resolute application of well-tested mechanical laws, and experimental

†This is still the case with the German word *Wissenschaft* which simply means 'body of knowledge'.

‡See [Billington 1985; Blockley 1980; Harris 1975; Holgate 1986; Mainstone 1973; Nervi 1945; Torroja 1967].

> results to the somewhat difficult problem offered by the construction of so large a bridge. [Baker 1882:433]

Hardly a case of design deriving 'little aid from previous experience'. What is astonishing is how an eminent James Forrest lecturer could ever have made the claims he did.

2

Concerning the history of structural engineering

The way in which any history of structural engineering and design is presented will clearly depend upon the way in which a writer conceives of the nature of the subjects in the present day. Not surprisingly then, the misunderstandings associated with seeing engineering in terms of theory and practice which were discussed in Chapter 1, also find their way into the history books. The consequences of these misunderstandings are further aggravated by the more general problems which arise when the history of a subject is written by enthusiasts of that subject rather than by trained historians: this is generally the case with engineering.

During the course of this book I shall be trying to show, not only how our understanding of the nature of modern structural design is affected by the way we are taught it and by our knowledge and understanding of its history, but also vice versa. Chapter 1 sought to identify some problems concerning our perception of the very nature of structural engineering design: here we consider several related difficulties which can arise when we start looking at the history of the subject. These will be dealt with under a number of headings:

- the separation of the histories of theory and practice;
- some difficulties in the history of engineering science;
- the changing meaning of words;
- using modern concepts when looking at the past;
- the artificial division of history into two ages;
- rendering the history of design invisible.

2.1 CONVENTIONAL HISTORIES OF ENGINEERING

The general adoption of an implicit theory/practice classification when discussing civil and structural engineering leads, not surprisingly, to their histories being presented similarly—often entirely separating theory and practice.

The history of the practical aspects tends to concentrate upon technological matters related to the fashioning of materials into components and their assembly in construction processes. For the relatively recent past much of this area also falls within the realm of industrial archaeology. The list of references contains several of the large number of works devoted to the technological aspects of the history of engineering including such classics as:

Encyclopaedia of Civil Engineering	[Cresy 1847]
Engineers and Engineering in the Renaissance	[Parsons 1939]
A History of Civil Engineering	[Straub 1952]
History of Technology	[Singer *et al.* 1954-58]
The Construction of Gothic Cathedrals	[Fitchen 1961]
An Illustrated History of Civil Engineering	[Pannell 1964]
The Masterbuilders	[Cowan 1977b]
Science and Building	[Cowan 1977c]

The history of theory is treated in several of the above list and also in the following, more specific, works:

History of the Theory of Elasticity	[Todhunter & Pearson 1886]
Les origines de la statique	[Duhem 1905]
Vorlesungen über Ingenieurwissenschaften	[Mehrtens 1903–23]
The Development of Structural Theory	[Hamilton 1952]
History of Strength of Materials	[Timoshenko 1953]
An Historical Outline of Architectural Science	[Cowan 1977a]
A History of Theory of Structures	[Charlton 1982]

When discussing the history of theory, works such as these deal, principally, with the branch of mathematics known as statics and its use in the engineering sciences of elasticity and strength of materials. It is these bodies of knowledge which are used by engineering scientists in their experimental investigations. They also form the basis of much structural analysis used by modern designers. Developments in the subjects are traced in academic writings from their 'invention' (usually by mathematicians or physicists), through to their appearance in the textbooks of civil and structural engineering science.

Such an approach does little to put the written works of mathematicians and scientists into the context of practical engineering. It also encourages the misleading implication that persons such as Leonardo da Vinci, Stevin, Newton, Galileo, Euler and Coulomb fit into the history of engineering at the time when their various works were written. This was, in fact, seldom the case: it usually took fifty years or so, and often very much longer, for the work of such men to find any practical use at all [Truesdell 1960, 1967, 1968]. As far as this approach tries to relate the histories of theory and practice at all, it tends to do so simply by placing them side by side and assuming that similar places and dates are all that is needed to connect them. Furthermore, little is made of the distinction between using the theory to help analyse an existing structure, which might be showing signs of distress and be in need of remedial work, and the use of mathematics and science by designers before a structure has been built.

The invention of the triangle of forces is generally credited to Simon Stevin (c. 1585), though Leonardo, as so often, had already hinted at the idea (a century earlier) [Straub 1952:54]. It was only in the 1690s, however, that we see the first use of this mathematical technique, by Lahire, to help in the analysis of masonary arches [Timoshenko 1953:63]. Lahire's ideas were first incorporated into a manual for engineers by Bélidor in 1729 and it is more than likely that engineers then began to use these ideas to help their design of some new structures, at least in France, although no actual design calculations seem to have survived from this time. For the calculation of forces in a truss, a further 50 years or so had to pass before the books on roof carpentry first mentioned the possible usefulness of the triangle of forces and it was as late as the 1840s when the idea was taken up by the designers of truss bridges [Timoshenko 1953:186]. Of greatest significance in the history of arch and truss design is the fact that developments in the branch of mathematics called statics and in other structural theory did not lead to the invention of new types of structure. But more of that anon; for now, a few words taken from the editorial introducing a review of an early book on statics:

> The magnificent structures which the present generation have seen commenced and completed—the admirable works of modern engineering which have so largely increased the value of our island for all the economic purposes of life . . . —all the greatest results of modern industry, have been created by mechanical contrivance and experiment, little aided, it must be confessed, by science. In making this observation, we intend the word *science* to be taken in its true sense—*systematic knowledge*. [Anon 1854:134]

2.2 THE HISTORY OF ENGINEERING SCIENCE

Accounts of the histories of theory and engineering science, such as those mentioned in §2.1, also raise a number of problems which are not specifically related to the theory/practice classification: indeed, they are encountered in dealing with the history of other branches of science such as physics and chemistry.

The history of engineering science (or 'theory' as it is often called) is usually restricted to only those contributions which, from a present-day viewpoint, are seen to be antecedents of currently accepted scientific beliefs. This approach implies a model of historical development which is like a river, with many sources and tributaries gradually merging into the ever-growing main stream. The task of the historian thereby becomes the tracing of the various tributaries back to their sources. This view of history has the disadvantage, however, that past achievements are seen as important because they, in some way, had anticipated the future course of development—in other words they were 'correct'. Other early work, which nowadays can be seen to have led up a blind alley, is judged to be of less worth because it was 'wrong' and therefore tends to be ignored. There is thus a tendency to overlook the influence, in its own day, of work which is now judged to have been wrong.

While the history of engineering science is not able to provide such spectacular examples as the caloric theory of heat and the substance called phlogiston, they do exist. Two of the many which can be found are the once common beliefs that the strength of a rope decreases with its length [Galileo 1638:121] and that bending a

beam until fracture causes the entire cross-section to go into tension. In fact, both of these 'incorrect' beliefs were extremely useful to 'practical' men (see Chapter 10).

Even more significant than the overlooking of individual 'incorrect' theories is the complete failure of all histories of engineering to mention other theories purporting to explain the workings of nature which did not involve the modern notions of statics and mechanics. While such theories have perhaps had little influence on engineers since 1800, they certainly did influence the work of the designers of buildings (be they called architects or engineers) before that time, at least as far back as Greek times.

A further widespread belief about the historical development of science is that it has been, and is, a wholely logical process. In the 19th and early 20th centuries it was believed to develop by means of a scientific method based upon inductive logic. More recently, through the work of Popper, the hypothetico-deductive model has become popular.

In this book a more Darwinian model is preferred in which progress is not seen as teleological. This type of approach has been developed mainly by the philosophers Kuhn and Feyerabend and will be elaborated in Chapter 9. Its principal advantage is that it not only acknowledges the methodical aspects of science but also its dependence upon highly individual and subjective matters. The way we think, and even what we can think, are utterly dependent upon our attidudes and beliefs, upon our language and the concepts it can express. It is thus possible for different people to have equally valid, yet logically incompatible, interpretations of the world.

2.3 THE CHANGING MEANING OF WORDS

When reading old texts one is often struck by words which have changed their meaning. Such changes can give valuable insight into how people at a particular time saw, both literally and figuratively, the structures around them. For instance, in the middle of the last century, bridges built on the principle of a beam strengthened by trussing rods beneath, were called suspension bridges; similarly some bridges which now would be called truss bridges often used to be called arch bridges, according to their geometry rather than the structural actions involved. These matters will be discussed in more detail in Chapter 7.

However, it is also possible to fail to notice that certain words in an old text which might appear familiar have, in fact, changed their meanings considerably. Of particular relevance is the way in which several of the words crucial to the discussion of the nature of engineering and design have changed their meanings. A closer look at some of these changes will help to explain how a few modern misunderstandings might have arisen.

Prior to the mid-18th century, the word 'science' denoted a body of knowledge, as illustrated by Aristotle's definition (§1.5). It had no specific connotations of experimentation, of the forming and testing of hypotheses or of a scientific method, as we now understand it. By the 19th century the word had also come to imply an understanding of the body of knowledge or, rather, the use of the body of knowledge to help understand and explain the world around us.

By this time the word was also used in conjunction with others to give much more specific connotations. Some examples, which will not concern us here, are 'abstract

science' and 'natural science' [Herschel 1830:18] and 'theoretical science', 'practical science', 'physical science' and 'speculative science' [Rankine 1856]. The word 'scientist' was first coined only in 1837 by William Whewell in his book *The Philosophy of the Inductive Sciences* [Whewell 1837].

The meaning of the word 'theory' also changed in the 19th century. It has its origins in the Greek words *theoria* meaning contemplation and *theoros*, a spectator. It is, by the way, surely significant that the two engineers who perhaps did most to introduce the use of theory into engineering design, Rondelet and Rankine, were both very concerned to clarify misunderstandings about the term and discussed its origins at some length (see Appendix 1). By the early 19th century it was common to treat the words 'science' and 'theory' as near synonyms. 'Science' was the body of raw facts in the world: 'theory' was the knowledge acquired by a person by looking and contemplation. Rondelet used the words in this manner in his work *The Art of Building*:

> La théorie est une science qui dirige toutes les opérations de la pratique. Cette science est le résultat de l'expérience et du raisonnement . . . [Rondelet 1812–17:v5.iv–v]

A passage by Sir John Herschel from his *Natural Philosophy* also illustrates this use of 'theory' as true fact:

> We might frequent printing-houses, and form a theory of printing, and having worked our way up to the point where the mechanical action commenced (the boiler of the steam engine), and verified it by taking it to pieces, and putting together again, the train of wheels and the presses, and by sound theoretical examination of all the transfers of motion from one part to another; we should, at length, pronounce our theory good, and declare that we understood printing thoroughly. [Herschel 1830:194]

Herschel then goes on, however, to contrast this older use of the word with what he identifies as a new, more specific, meaning it seemed to him, recently to have acquired: this was as a synonym for 'hypothesis'—a possible explanation for something not known, and as an aid to understanding. Thus, the idea of 'a theory' (with the article) was born and acquired its principal modern meaning. Incorporating of the idea of hypothesis was a significant development, for it meant that 'theory' changed from denoting knowledge as true fact, to denoting what only might be true. This change did not go without comment:

> Theory . . . is the name under which a class of operations is generalized, and their principle enunciated . . . Practice, unaided by theory, has only a *chance* of being right; whereas theory can never be wrong . . . The employment of [the word 'theory'] to denote that sort of philosophical guesswork called *hypothesis* is unphilosophical, although often *practised*. Hypothesis and theory are essentially different; the hypothesis *may be* correct, but the theory is essentially true. [Anon 1842:1]

With the above discussions in mind, it is worth referring, in passing, to two sources which provide good illustrations of the changes of use of the terms 'theory' and 'science' and the problems which can beset the interpretation of writings of the past. The two texts are Vitruvius' *Ten Books on Architecture* (c. 25 BC) and the proceedings of the inquiry into the design of Milan Cathedral (1391/2).

The first printed version of Vitruvius was published in Italy, in Latin, in 1486. It was first translated into French by Claude Perrault, an architect, in 1684. The first transla-

tion into English from the Latin was in 1791. Around 1812, the engineer Rondelet published annotated extracts in his book *The Art of Building*. Since then there have been three further translations into English, in 1826, 1914 and 1931.† These are of interest for several reasons.

- they illustrate almost incredible differences between several translations, due partly to the changing meanings of some of the words;
- they illustrate the contemporary attitudes of the translators to the job of the architect or engineer;
- they illustrate the doubt which three early translators felt about the adequacy of the words 'theory' and 'practice' in extracting the true meaning of Vitruvius' Latin; they included an architect (Perrault 1684) and an engineer (Rondelet 1812);
- they illustrate the particular inadequacies of translations by some classics scholars;
- they give examples of the frequent exhortations to combine 'theory' and 'practice' long before any engineering theory, in the modern sense of the word, existed.

The second example concerns just two phrases from the inquiry into the design of the partly built Milan Cathedral—*scientia est unum et ars est aliud* and *ars sine scientia nihil est*. The first of these was quoted by the Italian designers of the cathedral; the second was the retort by the French cathedral designer, Mignot, who had been called as an expert witness at the inquiry. In discussing the meanings of these two phrases, both of the major commentators [Ackerman 1949; Frankl 1960] have translated these phrases as *'science is one thing and art is another'* and *'art without science is nothing'*.‡ In the light of Aristotle's definitions and the discussion in this chapter, a more realistic interpretation might be obtained by translating *'scientia'* and *'ars'* respectively as 'engineering science' and 'engineering design'.

2.4 DESCRIBING THE PAST USING MODERN CONCEPTS

Just as it is easy to make the inadvertent error of giving a modern meaning to words written some time ago, so it is possible make bold and inaccurate inferences about the past by using concepts appropriate to our modern understanding of the world and methods of designing structures.

It is nowadays virtually impossible for a 20th century engineer to look at a Roman aqueduct, the Pantheon, or a Gothic cathedral without 'seeing' statical loads, lines of thrust and overturning moments at work. It is, therefore, extremely easy and dangerous to suggest or even to infer that the 2nd or 12th century engineers and masons saw their works in these terms. Nevertheless, it does happen:

> Untersucht man . . . die Sophienkirche oder das Pantheon statisch, so entdeckt man eine sorgsame Ausnutzung der verschiedenen Baustoffestigkeiten, wie sie unter

† Some relevant excerpts from these and other translations are given in Appendix 1.
‡ See §2.4 for Frankl's discussion.

> Anwendung einfacher Regeln für die Bemessung der einzelnen Bauglieder nicht erreicht werden könnte. Von seinen Zeitgenossen wurde der eine Erbauer, Anthemios, als Mathematiker und Mechaniker gerühmt. Das kann nichts anderes bedeuten, daß er für seine Bauten statische Untersuchungen anstellte.† [Hertwig 1934:90]

> The Italians [in the 13th century inquiry into the design of Milan cathedral] say: *Scientia est unum et ars est aliud.* That is, theory is one thing and practice is another. By that is implied: we have no exact theory of statics and so we build on the basis of our experience within the domain of practice. Mignot retorts: *Ars sine scientia nihil est.* That is, practice, too, requires theory, namely knowledge of the laws of statics.‡ This latter Mignot could not express clearly, since he had very little idea of the laws of statics. The Italians reply to this, in a rearguard action: Practice is indeed nothing without theory, but we have the theory of statics and dynamics of Aristotle on our side. [Frankl 1960:79]

Sometimes the inference is more subtle:

> The fruits of statics, new and old, are to be seen all around us. The ethereal grace of a Gothic cathedral . . . will, I hope, continue to catch at the heart-strings of youth and bring them to learn the power of statics. [Pugsley 1979:168]

Anachronisms such as these are not, of course, confined to the history of engineering. Thomas Kuhn relates vividly his own breakthrough in understanding the physics of Aristotle when he first saw the problems (e.g. the inertia of a moving body) and hence the explanations, as they might have appeared to a Greek rather than how the problems appear to us [Kuhn 1977:xi].

2.5 THE 'TWO AGE' VIEW OF ENGINEERING HISTORY

An inevitable consequence of viewing engineering in terms of theory and practice and of seeing design as 'putting theory into practice' is that it becomes important for the historian to try to answer the question 'when was theory first put into practice?'. Indeed, many writers on the history of engineering have tackled this question and given their answers. If they had all come to approximately the same answer, there would be little cause for concern. However, a selection of the dates cited by different authors must lead the reader to conclude that there seems indeed to be a problem, for they range over three centuries:

16th century	[Hertwig 1941]
the 1670s	[Dorn 1970]
the 1690s	[Hamilton 1952; Timoshenko 1953]
1742	[Mainstone 1959]
1816	[Cowan 1977c]
1848	[Anderson 1893]

† If one undertakes a statical investigation of Hagia Sophia [in Istanbul] or the Pantheon [in Rome], one discovers an exploitation of the properties of the various building materials which is so efficient that it could not have been achieved by the use of simple rules for the sizes of the individual members. One of the builders was famous amongst his contemporaries as a mathematician and mechanic. That can only mean that he used statical investigations for his building projects.

‡ In fact, the Latin text refers earlier to *'scientia geometriae'*. For further discussion of Gothic design, see §12.3.

Our attention would seem to be diverted to a different question: 'Why have these authors given such widely spread dates?'. Recalling the discussion in Chapter 1, the answer to this question is likely to depend on what, precisely, is meant by 'putting theory into practice'.

The trouble is that the question as to when theory was first applied in practice is ambiguous: it is a 'loaded question' containing hidden assumptions. First of all, theory is presumed to be the earliest antecedents of modern statics, elasticity and strength of materials. Other meanings of the word are ignored. Secondly, the question presumes a clear definition of the term practice. Thirdly, it assumes that it is clear what is meant by 'applying theory in practice"; and fourthly, the question suggests that 'practice' which follows from 'the application of theory' is different from 'practice' which arises in some other manner.

It will also follow that *any* attempt at establishing a correct date will imply a division of history into two periods or ages, one before 'the first application of theory' and one after. Underlying this 'two-age' view of history are several implications which lead to significant problems.

It is nowadays a widespread assumption that, since the supposed transition, whenever it may have been, the whole approach to engineering design has been different from how it was before. It is supposed that, before the transition, design was based solely upon experience, rules of thumb, trial and error, or 'merely by what may be called trained intuition' [Straub 1952:7]. Since the transition, design is presumed to have been based upon theory, to be rational and scientific, and no longer to rely upon trial and error, rules of thumb, experience and intuition. It is also implied that such design is better than design before the transition—better because it is 'scientific' and because it uses theory and that developments in design were considerably improved and accelerated solely as a result of using theory.

Yet, such suppositions and implications are at great variance with historical facts. Much progress nowadays still proceeds by trial and error, both in experimental conditions and, occasionally, in service in the form of more or less serious accidents [Smith 1976]. And on the other side, it is hardly fair to imagine Roman and Gothic engineers behaving like two-year-olds surrounded by a pile of bricks! There is no evidence that they were any less rational or intuitively competent than we are today. Evidence based on the history of inventions and, more recently, of patents awarded, suggests that modern man is no more inventive than his counterpart in the Middle Ages [White 1940].

Concerning the apparently rapid progress of the last century or so, it is misleading to compare this directly with progress in earlier periods of history. It is not unreasonable to suppose that the rate of progress in structural engineering, especially due to 'trial and error' (and 'trial and success'), is likely to be dependent on the cumulative number of structures built. In other words, it would be dependent not only upon time but upon human activity during a particular period. Thus, taking into account only the increasing population (of Europe), and regarding history in units of man-years rather than simply of years, one year in the 1980s is equivalent to three years in the 1850s, six in the 1760s and fifteen in the 13th century. If we also take into account economic factors and the ease with which people in different places could communicate, it is likely that the differences between the recent past, the 19th century and the 13th century would be even greater. These facts clearly minimize the supposed contribution to progress made by the use of science and theory.

Finally, two further facts should be mentioned. First, there have been many instances of authors extolling the benefits of bridging the gap between theory and practice long before even the earliest dates for the supposed 'first application of theory in practice" given at the start of this section (e.g. [Vitruvius c. 25 BC; Alberti 1485]). Secondly, there are nowadays still instances of the use of 'rules of thumb', 'incorrect theories' and 'trial and error' more than a century after the latest date assigned to the supposed first application of theory in practice, after which time such practices are supposed to have died out.

2.6 THE HISTORY OF ENGINEERING DESIGN

There remains perhaps the most serious problem with the conventional approach to history couched in terms of theory and practice. It fails to ask, let alone answer, some extremely important questions concerning the history of engineering, for instance:

- how did engineers (or masterbuilders or architects) design their works?†
- before the existence of modern theory, what did engineers use in its stead?
- when practising design engineers made use of theory, precisely how did they use it?
- when they did (or do) not use theory, why not?
- what use was, and is, made of other bodies of knowledge in addition to theory?

Put another way, the conventional theory/practice classification renders the activity of design invisible in the past, just as it also tends to render it invisible in the present. There are of course exceptions to this generalization. A few writers have identifed 'rules of thumb' and 'incorrect' theory as antecedents of our modern, supposedly 'correct' engineering 'theory' [Mainstone 1959, 1968; Cowan 1977a; Blockley 1980]. However, their main purpose in mentioning them has been to contrast them with modern 'scientific' methods. The approach advocated here is to concentrate upon the similarities rather than upon the differences and to aim to present the history of structural engineeering design as a continuum, not as two disconnected parts. While this approach has, perhaps, not previously been applied to structural engineering and design, it is well established in other areas of the history of science and technology, especially following the work of Alexandre Koyré (see [Koyré 1948; Layton 1974]).

A few authors have discussed some of the problems of the practising engineer more effectively than simply placing the histories of theory and practice in close proximity and thus implying their interrelation [Meyer 1909; Hamilton 1951; Dorn 1970; Reynolds 1973]. However, they have tended to approach the problem from the point of view of the scientist looking for ways of putting theory into practice or 'applying science'. One author has even advocated this approach as a general historical method in a paper entitled 'The Genesis of Engineering: Theory and Practice' [Hall 1969]. This does not, however, tell the story as seen through the eyes of the engineers themselves.

†The design of Gothic cathedrals has attracted most interest in the history of design (see §12.3 and §13.2). For a rare look at the design of a more modern structure, the Eadr Bridge (1874), see [Kouwenhoven 1982].

Finally in this chapter, I should mention again the dangers of interpreting the past in terms of modern concepts, this time in the context of design and construction. It is nowadays all too easy to assume that, before the availability of modern engineering theory, not only were structures designed and constructed differently, but also that the whole attitude to design must have been different (and even inferior) since we nowadays would solve their problems differently. It should, however, be remembered that people have not always had our zealous concern for economy in materials and time and our predilection for certain structural types which are favoured for reasons associated with the economic climate, ease of construction or ease of mathematical analysis. In earlier times the world appeared otherwise.

It is, frankly, astonishing just how many engineers are unconsciously disparaging of the achievements of engineers in earlier ages. The belief that we are better than they were is, sadly, rampant. It is worth reminding ourselves that in times to come we, too, are likely to be believed to have lived in an extraordinarily unenlightened period in the bad old days of the past. Perhaps engineers are frightened of looking at history too closely because they might find that they are not better than their ancestors.

3

Concerning other matters related to structural engineering

3.1 PROGRESS AND RESEARCH

The supposed dependence of practice upon theory or science illustrated in Chapter 1, is of especial consequence for the historical development of engineering—what is often called progress. It would suggest that it is theory and science which leads practice in the progress of engineering. That this view is widely held, can be verified by perusal of, for instance, the many Presidential Addresses and the James Forrest Lectures on the 'Interdependence of Abstract Science and Engineering' in the Proceedings of the Institution of Civil Engineers. Since their beginning in 1893, however, only one James Forrest lecturer has challenged the prevailing view that it is engineering which benefits from the achievements of science. He gives several examples of the contribution which engineering has made to science and suggests that:

> we should bestow upon the scientific element a more cautious and discriminating appraisement [to quell the] unbroken chorus eulogizing the achievements of science [the result of which has been] that the eyes of men are dazzled, their ears stunned, their wills paralysed, and their minds hypnotized. [Maunsell 1951:362]

A few other eminent writers on engineering have also directly challenged the common view that it is theory which leads practice:

> It is imperatively necessary that effects which cannot be calculated exactly should be taken into account in constructions, and it is in this sense that elastic theory is at this time behind engineering practice. [Love 1892:109]

> History shows that there is hardly a single concept of practical importance in the field of structural engineering that was not instinctively anticipated and used with success in design and construction by individuals or groups of engineers many centuries before applied mechanics came into existence. [Terzaghi & Peck 1948:253; reproduced by permission; © 1948 John Wiley & Sons]

The question as to whether theory or practice comes first would, perhaps, be largely academic were it not for the fact that the popular notion that it is theory which leads materially affects the decision as to where research is considered to be most effective and, hence, to whom research funding is granted:

> The scientist . . . sought to exploit his own ideas directly. He had the . . . advantage that it was only his successes that were reported, while the engineer was expected to succeed and it was mainly his failures that made news. The idea therefore grew in the public mind and in government circles that it was the scientist who was the prime initiator of new technology . . . .
>
> The process of industrial innovation and advancing technology were seen as a progression from pure science, through applied research and development to exploitation by industry and, as the originator and the one who best understood the fundamental factors involved, the scientist was seen as the appropriate planner of the national programme for all research . . . .
>
> In practice [industrial innovation] is a much less tidy process. Most innovations still originate in industry and most industrial research is undertaken to make the inventions work properly. The relationship between science, industrial research and industry, in fact operates in both directions as a kind of dialogue in which each makes a contribution in turn . . . .
>
> It is just as important and rather more difficult to keep research up-to-date with practice as it is to keep practice up-to-date with research. [Collins 1978:202-3]

There is, in fact, no lack of evidence that research into theory and science does not lead to practical applications (see, for instance, [Price 1968; Gibbons & Johnson 1970; Jobst 1974; Langrish 1974; Layton 1976]). The general conclusion is that:

> science seems to accumulate mainly on the basis of past science, and technology primarily on the basis of past technology. [Mulkay 1979].

The question as to just who does what research (and why) is far too large and contentious an issue to deal with here. However, as will become clear during this book, there are a great many subjects which seem to fall between two stools, be they the stools of theory and practice, or university and industry.

There are also many (probably libellous) stories which could be told of the failure by ivory tower researchers to serve industry's research needs and of grimy-handed engineers failing to take up the pearls of wisdom generated in universities. One structural engineer who had gone to a university for help in solving a design problem recounted wryly that the researchers were still trying to solve the problem 'definitively' when the building he had designed was being demolished after many years of good use.

Whatever one believes the role of university-based research is or should be, many misunderstandings would seem to rest upon the misleading notion that engineering is a matter of putting theory into practice or applying science. It would perhaps benefit both sides of the debate to consider carefully what is meant by progress and either to agree or to agree to differ. One should also consider carefully the views of researchers in engineering (and other) sciences who often justify their research in terms of its practical application. There have, after all, been many inquiries conducted as to why industry has been reluctant to take up the fruits of university research.

3.2 EDUCATION

The twin notions that theory governs practice and that engineering design is a matter of putting theory into practice, have important implications for the education of future designers. The academic environment is not directly concerned with the realms of practice and designing real structures and works of civil engineering. The primary emphasis is, therefore, the teaching of the theory upon which practice is supposed to depend. It usually goes without question that this is a suitable preparation for its being put into practice.

This situation might be satisfactory if it were not for the occasional voices of dissent, especially from the pens of several eminent engineers. The emphasis of the complaints varies slightly between writers, but the general message is that students concentrate too much on the quantitative and *theoretical* aspects of engineering and too little on certain other aspects. They consequently graduate lacking real understanding, for instance, of the physical behaviour of structures, not appreciating how the theory they have learnt can be used, and having little idea of what design really is:

> The pre-eminence given to mathematics in our schools of engineering, the purely analytical basis of the theory of elasticity, and its intrinsic difficulties persuade the young student that there is limitless potency in theoretical calculations, and give him blind faith in their results. Under these conditions neither students nor teachers try to understand and to feel intuitively the physical reality of a structure, how it moves under a load, and how the various elements of a statically indeterminate system react among themselves. Today everything is done by theoretical calculations. That student is rated best who best knows how to set up and solve mathematical equations . . . .
>
> The mastering of structural knowledge is not synonymous with a knowledge of those mathematical developments which today constitute the so-called theory of structures. It is the result of a physical understanding of the complex behaviour of a building, coupled with an intuitive interpretation of theoretical calculations. [Nervi 1956:16,24]

> Unless theoretical knowledge leads ultimately to an understanding of structural action and unless this understanding can be interpreted in terms of structural design, we have achieved only futility. [Cross 1963:97]

> Many of our present graduates do not have that familiarity with structural behaviour which should be demanded from them if they are to meet minimal professional standards. [Brohn & Cowan 1977:9]

> The teaching of theory of structures in this country has concentrated throughout this century on theory rather than design. [Pugsley 1980:50]

> If students are taught solely the theory which lies behind a practice of which they learn nothing, they conclude that only the theory is important. [Harris 1980:409]

> The student must acquire a sound, reliable grasp of the principles of structural design which will allow him to judge what are the central issues; such as what is the relationship between the theoretical model of the structure and its real behaviour and the requirements of the code of practice. [Dunican 1981:1]

Nowadays the teaching of virtually the whole of civil and structural engineering is approached by means of mathematical models of reality—what is usually referred to as theory and structural analysis. Yet, the relationship between such models and real

structures, is seldom discussed. Thus the student comes to believe and finally even to 'know' that the common assumptions behind the models are 'true'. Thus, all engineering materials become Hookean, pin-joints are frictionless and, in a beam, plane sections do remain plane, not only in the mathematical models, but in the real structures too. Emphasis is given to the manipulation of a mathematical model and its solution in terms of stresses, deflexions and so on. But the more important and fundamental problem of *creating* a mathematical model which represents, in an appropriate way, the behaviour of a structure, is generally ignored. It is implied that this is a matter of little difficulty and small concern.

The idea is thus encouraged that a structure has a single, unique model and, conversely, that a given model has a single and unique counterpart in the real world. Seldom is any mention made of the possibility of there being alternative models and that criteria are needed by which the alternatives can be judged and evaluated.

All engineering courses contain elements of practical work which purport to develop an understanding of the relationship between theory and practice (mathematical model and real structure), such as when the measured stresses, strains and deflexions of a pin-jointed truss, or of a rigid jointed frame are compared with the results predicted by theory and, usually, a close correlation of the two is reported. Yet, such experiments are not in any way testing the laws of statics (which are mathematical and necessarily true), nor the actual behaviour of a structure (which is usually overlooked in favour of the 'correct' solution given by the formulae). A greater value of such an experiment could be to highlight the inadequacy of the mathematical model in accurately representing the behaviour of a real structure, while yet emphasizing the use to which such an inadequate model could nevertheless be put when designing similar structures.

The assumed uniqueness of the relationship between a model and its real-world counterpart has the even more serious consequence, reinforced by dozens of examples and examination questions which yield one correct solution, that the whole process of designing in engineering is believed to be precise and exact, and each problem would seem to have a unique solution—the 'one best way'.

The result of all this is that students, and indeed other non-engineering-designers such as architects and the public at large, come to believe that stresses, strains and deflexions in actual structures can be accurately predicted to any number of decimal places, and consequently, that such calculation techniques are infallible in design. So complete is this indoctrination that young designers can even give calculated results more credance than even their most basic intuition. Many design engineers can relate anecdotes such as the tall building calculated to be bending into the wind for half its height, or the calculations which 'proved' that a bridge design was unsafe and would lead to collapse, despite the fact that the bridge had already been constructed and was in full use [Nervi 1956:15].

Such problems are likely to become more frequent and serious with the the increasing use of computers in analysis and design (see [Abdelmigid 1975; Fanelli & Giuseppetti 1980; MacLeod 1988]). The users of computers nowadays do not generally write the programs themselves and are thus likely to be unaware of any assumptions made. It is also becoming more likely that users are less inclined or less able to check the output of computers, even approximately; and as it becomes easier to feed a complex structure into a computer, so people are becoming less and less inclined to think

about its behaviour beforehand, as used to be the case when it was in a designer's interests to keep the calculations as simple as possible.

In summary, students are now told mainly about the mathematics and engineering science relevant to engineering works, but not how to use this knowledge in design. Nor are they taught the importance of other types of engineering knowledge in design, such as a qualitative understanding of structural behaviour, precedent, empirical data and rules ('rules of thumb'). And lastly, they are poorly educated as to the limitations of theory, how and when its efficacy in design might be suspect, and when it might need to be supported, for instance, by tests on physical models.

3.3 AESTHETICS AND CRITICISM

In the same way that many engineers have had ambiguous feelings about whether the design of civil and structural engineering artefacts is a science or an art, so they have had ambivalent feelings about the place of aesthetics and criticism, both in their work and in the education and training which is meant to prepare people for the profession. It would seem likely that these facts are not unrelated. If the work of the designer is seen as somehow putting into reality the theories of the scientist, it could be argued that there is no room for variation in the manner in which this is executed—either a design has been carried out in a scientific manner, drawing upon the truths of science and mathematics, or it has not. This type of view is seldom stated so bluntly, but it is frequently implicit in the attitudes of many engineers (especially engineering scientists) and most students, during their education, receive the unambiguous impression that all questions in engineering have answers which are either right or wrong, which are unique and which do not admit of debate.

There are others, however, who hold that engineering design is an art and, in common with other human skills, can be executed more or less well. Most engineering designers have well-developed ideas both of good design, in a technical sense, and of the beauty of a designed object—its aesthetic quality in the purely visual sense of aesthetic. There is even the old adage, which subtly and succinctly blends the purely visual and the technical appreciation of an artefact—'if a design looks right, it is right'; though, as with all generalizations, there are plenty of exceptions to prove the rule. Then again, there is the idea of 'engineering judgement' the value of which has already been mentioned in citations by several eminent engineers in Chapter 1.

Yet it is curious that the idea that there is good and bad design and good and bad designers has had so little impact on engineering culture. Very little has been written on the subject itself, only a handful of engineering biographies seriously address the reasons why a certain engineer was great, and the whole notion of criticizing works of engineering is considered almost a taboo subject, both during education and in professional life.

How different all this is from the closely related discipline of architecture where whole library shelves are devoted to aesthetics and criticism and it plays a central part in education for the profession. Looking further afield, the same can be said of many other 'artistic' skills—music, painting, poetry, literature. It appears to be widely believed in all these other fields that, in order to improve and maintain quality (in its most general sense), it is both desirable and necessary to have the efforts of artists

publicly appraised by their peers, and even by professional critics. While in the field of engineering it is now more and more the trend that the whole idea of quality is a matter for technical specifications of workmanship and, ultimately, the concern of lawyers. Recently it has even been found necessary to introduce Codes of Practice concerning the assurance of quality. Sadly, such a step is likely to lead designers more and more from the idea of self-generated excellence towards the attitude that the goal becomes the minimum level specified by a set of rules.

How much easier and more cost-effective it would be to instil in engineers the basic ideas of excellence and the ability to criticize and to appraise, as part of their education and training. It is far easier to make appeal to adults who have been educated to understand the fundamentals of good design, than it is to try to argue with people who have had a narrower education which introduces the tools of the trade with no feeling for how they can and should be used, let alone an emotional commitment to excellence and ideals.

At present, virtually the only idea of good design by structural engineers with which people are familiar are the annual awards made by the professional institutions and the trade associations representing the manufacturers of concrete, steel, timber and so on. While these awards have a certain value, it is unfortunate that the criteria by which the judgements are made are not more publicized and debated, especially if one of the purposes of the awards is to help to raise the general quality of structural engineering design. If we are to have better design, surely we need to look at how engineers choose between possible solutions to design problems and decide when something 'looks right' and develop a reliable sense of 'engineering judgement'. If we do not, how are we to know what we are doing is as good as it might be, whether as students or professionals?

In the remainder of this book I will try to show how alternative attitudes to the nature of engineering and design might demonstrate the value of (and even the need for) an open and critical appraisal of engineering structures and the process by which they are designed.

4

Towards a resolution of the problems

In the previous chapters I have suggested that a principal cause underlying a number of problems related to structural engineering is the conventional division of engineering into the categories of theory and practice. According to this view, the main issues in the philosophy of engineering are seen as the gap between theory and practice, and how this gap can be bridged.

We saw in the many citations in Chapter 1, that most people who have addressed the matter suggest or tacitly assume that there is some sort of causal link between theory and practice, that practice somehow depends upon theory. No-one, however, has yet managed to reveal the precise nature of this link—on the contrary, in fact. At a conference on 'The Interaction of Science and Technology in the Industrial Age' in 1973, a group of eminent historians and philosophers of technology agreed that there was no evidence of a logical dependency of engineering and technological knowledge upon science [Layton 1976].

At first sight this conclusion might appear to be a matter of great concern to engineers. However, on reflexion, it will be clear to most practising engineers that their world is not at all under threat, for they already knew intuitively that it did not depend on science, and the myth that it did has been perpetuated mainly by scientists and theoreticians. Unfortunately very few engineers have recorded these views for posterity and to guide the education of their heirs. In terms of self-publicity, the scientists have consistently been the winners.

4.1 BRIDGING THE GAP BETWEEN THEORY AND PRACTICE

As we have seen, the conventional way of dealing with the problem of the gap between theory and practice has been to talk of engineering and design as 'applying theory' and 'putting theory into practice', believing that using such phrases solves the problem. So popular are such phrases that their precise meaning is nowadays seldom discussed or questioned.

Typically, the results of academics' and researchers' work in engineering science is presented in a form which is believed to make it clear to their practising colleagues what to do with it. They discuss the theory and the practice of their laboratory experiments and tests, generally proclaim a close agreement between the two, and assume that the results will therefore be of immediate use to engineering designers. The pages of the Proceedings of the Institution of Civil Engineers (to take the longest-running example) have been filled with such papers for nearly 150 years. They have also, however, contained many a contribution to the discussion of such papers from 'practical men' who have complained at the inappropriateness of the scientists' work.

In general, this type of solution to the theory/practice gap has been unsatisfactory, since it has failed to help the engineering designer understand just how the theory can be used outside the laboratory. Simply using phrases such as 'putting theory into practice' without explaining them, does not constitute a useful bridge between theory and practice.

The most successful attempts at dealing with the problem of the purported gap between theory and practice have, in fact, been made by practising engineers rather than scientists. While the way in which their solutions have been expressed varies, they are all based upon idea that something else must be taken into account which is neither theory nor practice (the 18th century Frenchman Prony was one example [Picon 1988:127]). Of primary concern to engineers is the fact that theory and practice are different types of thing and relate to different situations, as illustrated, for instance, by this excerpt from a lecture entitled 'The Contrast of Practice and Theory':

> Practice [is] something wherein all the facts of a case are known, down to the minutest microscopic particular, and every possible consequence of every possible influence works itself out in due proportion; while Theory always, and unavoidably, starts with facts more or less wrong, and data consisting of a selection only, generally a very small one, of the real circumstances, with only a few of the consequences completely followed out, and the rest either ignored altogether, roughly guessed at, or merely fixed as lying within certain limits. [FitzGerald 1891:32]

The nature of the relation between theory and practice has often been expressed as advice to engineers on how to make use of theory and calculations. In general, care and even scepticism are recommended when making use of results of calculation and theory, and these should be modified by the experienced designer when felt appropriate: the words of Torroja, Nervi, Candela and Maillart, who counselled against the blind use of theory and calculations, will be recalled from Chapter 1. In fact, warnings about the use of theory and the importance of experience can be traced back rather further than one might at first expect. In one of the very first published works on the statical analyis of bridge trusses, in 1851, the German engineer, Schwedler, was already very clear:

> Vorstehende Bemerkungen sind nur gemacht worden, um anzudeuten, wie eine Theorie, die auf bestimmten Voraussetzungen basirt ist, nicht auf Bauausführungen angewendet werden kann, bevor man geprüft hat, ob auch sämmtliche Voraussetzungen bei dem Bauwerke gemacht werden können. Es wird sich im Gegentheil finden, daß die Theorie für jedes Bauwerk, je nach dem Material, dessen Elasticität, den Qwerschnit-

> ten der Theile, den Detailverbindungen, und noch mancherlei andern Sachen besonders rectificirt werden muss, wenn man nicht in Fehler verfallen will.
>
> Die Theorie gibt nur im Allgemeinen ein Schema, nach welchem die Stabilität des Bauwerkes durchdacht werden soll, dem einzelnen Baumeister bleibt es danach überlassen, in jedem besondern Falle dieses Schema mit seinen Gedanken auszufüllen.† [Schwedler 1851:167]

Even further back in history, and as the first of many parallels which will be drawn in this essay between modern and earlier times, the same message is found in a letter from Lorenz Lechler, the author of a design handbook for parts of Gothic cathedrals, to his son in the year 1516:

> Give to this writing [the handbook] careful attention, just as I have written it for you. However, it is not written in such a way that you should follow it in all things. For whatever seems to you that it can be better, then it is better, according to your own good thinking. [Lechler 1516, cited in Shelby & Mark 1979:115]

And even earlier, around 25 BC, Vitruvius offers similar advice to his readers:

> There is nothing to which an architect [building designer] should devote more thought than to the exact proportions of his building with reference to a certain part selected as the standard. After the standard of symmetry has been determined, and the proportionate dimensions adjusted by calculations, it is next the part of wisdom to consider the nature of the site, or questions of use or beauty, and modify the plan by diminutions or additions. [Vitruvius 7.2.1]

The approach of these solutions is attractive. It acknowledges the expertise of the engineer and indicates that it is ultimately his decision to use or not to use the results of theory, or to use them with care. This process will be familiar to many engineering designers, who work in this way every day.

However, they do little to develop a deeper understanding of the nature of engineering. They imply the idea of putting theory into practice without elucidating what this process comprises or entails; and they beg another question: How is an engineer to know when and how to 'fill the scheme out with his own thoughts' or to take notice of 'his own good thinking'? While it is generally assumed that, from the theoretical standpoint, theory can be of considerable use in practice, and that, from the practical standpoint, practice can somehow be improved by a knowledge and use of theory, the approaches outlined above do not indicate how and why theory can be of use in practice. More generally, the possible role for theory in the practical side of engineering is not elaborated. It was this very issue which led to so much scepticism, especially in the last century, but also in the present one, about the value of theory to the practical engineer.

Both 'practical' and 'theoretical' men needed to be shown how to relate theory and practice and the person who, more specifically than any other, addressed this funda-

† The previous remarks have been made specifically to indicate that a theory, based upon certain assumptions, cannot be used in design until it has been tested whether the various assumptions can be made in a particular case. In fact, it will be found that the theory will have to be modified for each construction, according to the materials, their elasticity, the sections of the components, the joint details and sundry other things, if one is to avoid errors.

The theory gives only a general scheme in terms of which the stability of the structure should be thought through. It is up to each individual engineer to fill this scheme out with his own thoughts.

mental issue was W. J. M. Rankine. His solution, like that of the three writers cited above, was to postulate something distinct between theory and practice. His suggestion was the existence of a third type of knowledge, and consequently implied a third type of activity, intermediate between the other two. In taking this step he was attempting to specify, for the first time, the activity which is perhaps at the heart of engineering, what is now generally referred to as designing.

4.2 RANKINE'S 'HARMONY OF THEORY AND PRACTICE'

Rankine's contribution to engineering was enormous and is difficult to summarize briefly. Channell has appraised his work in 'A Unitary Technology: The Engineering Science of W.J.M. Rankine' [Channell 1975], from which the following citations are taken:

> [In the early 19th century] there was a social and institutional distinction between theory and practice which implied that engineering was either an adjunct of science or simple rule-of-thumb empiricism . . . .
>
> [Rankine] had a background in both theory and practice, but he found it was not enough simply to combine the two. Rather, he had to transcend the concepts of theory and practice and establish engineering science as a distinct and independent body of knowledge . . . .
>
> The formal creation of engineering science would transcend the traditional categories of theory and practice in such a way that it would not threaten the scientists, yet it would not duplicate what was learned through apprenticeship training . . . .
>
> Rankine's engineering work reflected a unitary approach to phenomena. A belief in unity excluded a distinction between theory and practice . . . His approach allowed him to reconceptualize engineering; rather than solving specific problems, Rankine sought the principles behind the phenomena and solved the problem in the most general form . . . This led to one of his most powerful tools for solving engineering problems—solution by analogy . . .
>
> [Rankine's] conceptualization of an ideal structure enabled [him] to establish the underlying unity behind the theory of structures . . . Two structures might outwardly appear different to the eye, but by reducing them to an equivalent set of points and forces, Rankine treated them essentially as equivalent structures. [Channell 1975:16,39,41,52,65]

Rankine was the first author (and engineer) who managed to draw upon his practical engineering experience and his knowledge of the theory of engineering and to demonstrate to others, academics and practical men alike, not only that the two could be related, but how this could be done. In his several written works in many fields of engineering, he indicated how apparently useless idealized concepts, such as precise forces acting at single (mathematical) points and geometry expressed by points and single lines, could be used to represent, in an idealized and simplified way, real loads and actual structural members.

Rankine was the first person who gave serious and clear attention to the process of creating a mathematical model of an actual structure. He was thus better able to convince practical men that, although statics and mechanics could never fully represent actual structures (which was always the main criticism), they could nevertheless be of considerable use.

Most importantly of all, he was also able to show the more academically minded how they could modify the results of their idealized mathematical work in such a way as to make them acceptable to the practical men. This he achieved by using the 'factor of safety' in a new way—its function was changed from one directly concerned with safety (i.e. designing a bridge to be able to carry twice the load it was ever expected to carry in reality) to one which was specifically to mask the consequences of designing by means of mathematical models (of the loads, the materials and the structure) which were known to be simplified and, therefore, inaccurate (see §14.4.2).

Rankine first introduced his new approach in an address at the opening of the newly formed Mechanical Science section of the British Association for the Advancement of Science in 1855:

> The study of scientific principles with a view to their practical application is a distinct art, requiring methods of its own . . . This kind of knowledge (intermediate between purely scientific and purely practical) . . . enables its possessor to plan a structure or machine for a given purpose without the necessity of copying some existing example—to compute the theoretical limit of strength and stability of a structure, or the efficiency of a machine of a particular kind—to ascertain by how far an actual structure or machine fails to attain that limit, and to discover the cause and the remedy of such shortcoming—to determine to what extent, in laying down principles for practical use, it is advantageous, for the sake of simplicity, to deviate from the exactness required by pure science; and to judge how far an existing practical rule is founded on reason, how far on mere custom, and how far on error. [Rankine 1855:201]

In the following year, Rankine gave his introductory lecture as Regius Professor of Civil Engineering at the University of Glasgow entitled 'The Harmony of Theory and Practice in Mechanics'. In choosing this title he was no doubt aiming to convince his audience by resorting to the popular terms 'theory' and 'practice', and sought to explain the fallacy behind their supposed inconsistency. It is worth citing at length from this lecture:

> 'Theory' and practice are of Greek origin: they carry our thoughts back to the time of those ancient philosophers by whom they were contrived; and by whom they were contrasted and placed in opposition, as denoting two conflicting and mutually inconsistent ideas . . . In physics and in mechanics [the Greeks'] notions were very generally pervaded by a great fallacy, which attained its complete and most mischievous development amongst the mediaeval schoolmen, and the remains of whose influence can be traced even in the present day—the fallacy of a double system of natural laws; one theoretical, geometrical, rational, discoverable by contemplation, applicable to celestial, aetherial, indestructible bodies, and being an object of the noble and liberal arts; the other practical, mechanical, empirical, discoverable by experience, applicable to terrestrial, gross, destructible bodies, and being an object of what were once called the vulgar and sordid arts.
>
> The so-called physical theories . . . being empty dreams, with but a trace of truth here and there, and at variance with the results of every-day observation on the surface of the planet we inhabit, were calculated to perpetuate the fallacy . . . Rational and practical mechanics (as Newton observes in his preface to the *Principia*) were considered as in a measure opposed to each other . . . .
>
> As a systematically avowed doctrine, there can be no doubt that the fallacy of a discrepancy between rational and physical mechanics came long since to an end; and that every well-informed and sane man, expressing a deliberate opinion upon the mutual relations of those two branches of science, would at once admit that they agree in their

> principles, and assist each other's progress, and that such distinction as exists between them arises from the difference of the *purposes* to which the same body of principles is applied.
>
> If this doctrine had as strong an influence over the actions of men as it now has over their reasonings, it would have been unnecessary for me to describe, so fully as I have done, the great scientific fallacy of the ancients . . . but, unfortunately, that discrepancy between theory and practice, which in sound physical and mechanical science is a delusion, has a real existence in the minds of men; and that fallacy, though rejected by their judgements, continues to exert an influence over their acts . . . .
>
> Mechanical knowledge may obviously be distinguished into three kinds: purely scientific knowledge,—purely practical knowledge,—and that intermediate kind of knowledge which relates to the application of scientific principles to practical purposes, and which arises from understanding the harmony of theory and practice. [Rankine 1856:4,9,17]

Rankine had identified a third kind of knowledge which was not the application of theory in practice but the application of scientific principles arising from 'understanding the harmony of theory and practice'. However, the subtlety of this distinction went un-noticed in many quarters. Even today, as Rankine observed in 1856, 'the great scientific fallacy . . . though rejected by [men's] judgement, continues to exert an influence over their acts'. Rankine made a further attempt to drive the point home in the introduction to his *Manual of the Steam Engine:*

> In the history of the mechanical art two modes of progress may be distinguished—the empirical and the scientific. Not the practical and the theoretic, for that distinction is fallacious. [Rankine 1859:xix]

Rankine may not have succeeded in getting his 'transcending of the concepts of theory and practice' accepted by the whole engineering community, but he did lay the foundations for the modern approach to engineering design (the word design only came to be used in its modern engineering sense towards the end of the last century) and showed how the theory of engineering can be put to use. It was also he who firmly established engineering science as an autonomous field of interest separate from physics and mathematics.

4.3 AN ALTERNATIVE CLASSIFICATION OF ENGINEERING

I hope to show how Rankine's approach to engineering can be developed and combined with some ideas from the history and philosophy to provide a better understanding of our subject and its history. Rankine demonstrated how the traditional concepts of theory and practice can be transcended by offering an alternative classification of the activities encompassed by the general terms 'science' and 'engineering'. This new classification was based upon the different aims or *purposes* of persons undertaking scientific and engineering activities, not upon the types of knowledge they used.

In more modern language, Rankine was trying to separate what we now call experimental engineering science from the body of knowledge ('science' in its original sense) upon which a designer draws, according to the different purposes to which each is dedicated. The first of these I shall called simply 'engineering science' and the

second 'engineering knowledge', and the two distinct purposes, or ends, are 'engineering science' and 'engineering design'. There is, of course, a further sense of the old idea of practice which has now been excluded—the manufacturing side of the industry. This can be conveniently referred to as 'construction'.

A further aim of my argument becomes, then, to show how this distinction between engineering science, design and construction, can provide the means of avoiding the problems concerning structural engineering which have been discussed in the previous chapters. Indeed, without any further discussion, two problems are already solved—two separate activities within engineering have been distinguished by their aims (engineering science and design) and the activity of design is no longer rendered invisible, as it tends to be by the theory/practice classification.

Engineering science has aims and methods similar to those of other, 'purer' sciences such as physics. The ultimate aim of both is understanding and explaining the world. They both make use of theories to explain observed phenomena and involve controlled laboratory experimentation in the testing of the hypotheses.†

Engineering design, on the other hand, is concerned primarily with the production of artefacts in conditions much less under control than those of the laboratory and with much less complete data. This is achieved by means of a particular type of knowledge and understanding of the world which is different from that appropriate to science. The main significance of distinguishing these two activities by their aims is that, because the aims are different, so the criteria according to which they are judged will also be different. The following few chapters will explore the aims of engineering design and the means by which they are achieved as a prelude to looking at the criteria used to assess success and progress.

† For a discussion of the functions of theories and the similarities and differences between pure and engineering sciences, see Appendix 2 and [Vicenti 1982].

5

The design procedure

A definition of design in structural and civil engineering which would seem to be satisfactory for our present purposes is:

> the determination of what is to be built and the preparation of the instructions necessary for building it. [Harris 1975:17]

In striving to achieve this goal, the designer has other aims:

> The factors which the engineer has in mind when designing are threefold; function, economy and safety. [Harris 1975:18]

Such a definition is a useful starting point, but if we are to explore and clarify the nature of design in structural engineering we need to analyse the process more deeply.

Design is often shown as a process comprising a series of linked activities, including feedback loops to represent the 'trial and error' nature of design. An example of this type of model is shown in Fig. 5.1 and is particularly useful to indicate the place of design within an entire project. For our present purposes, however, such a model is not very helpful since it masks precisely those parts of the design process which are of particular interest—the nature of the designer's contribution and the type of knowledge and skill he or she might bring to the process.

To understand the designer's role better we need a model which represents design from the designer's point of view. This can be achieved with a 'black box' model, with an input and an output. Such a model is shown in Fig. 5.2. The central activity is that of the designer, as encapsulated in the *design procedure* for a particular project. As input to the activity is the knowledge and experience of the designer; as output, there are the description and the justification of a proposed design. The process is influenced and regulated by a wide variety of factors. Before discussing the contents of the black box, the model will be explored in more detail by considering the separate components individually, starting with the output.

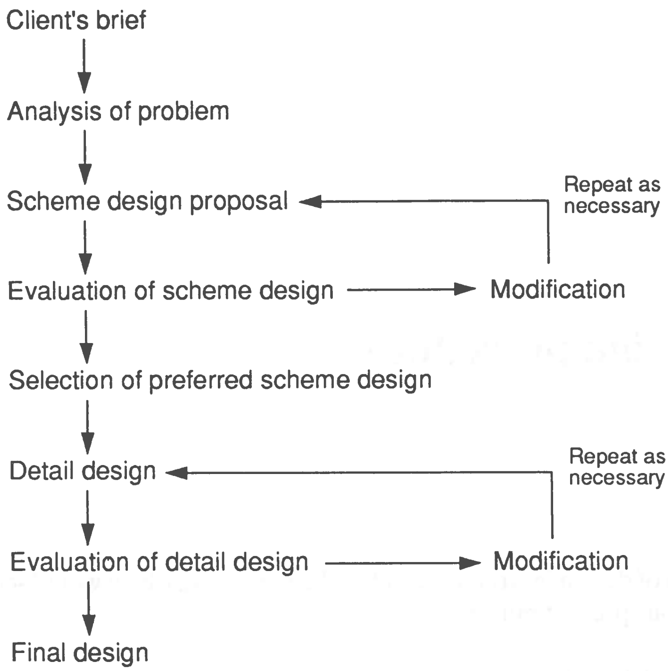


Fig. 5.1—A project-related model of the design process.

5.1 OUTPUT—DESCRIPTION AND JUSTIFICATION

As noted above, a primary aim of design is 'the preparation of the instructions necessary for building' something. Even more important, however, is the need to provide evidence that the proposed design will be adequate in a large variety of ways. These

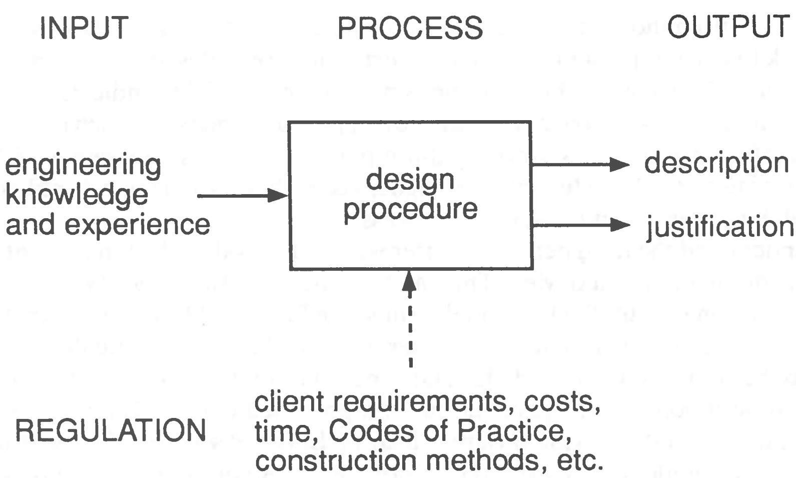


Fig. 5.2—A design-centred model of the design process.

two fundamental aims of design I call the 'description' and the 'justification' of a proposed design.

5.1.1 Description

A designer must be able to communicate his design to the people who are to make or build it. The precise manner in which designs are communicated varies considerably from product to product and from age to age. At a basic level two types of information need to be communicated—the geometry of the product and the material of which it should be made. Materials, which are not of particular concern here, are usually described simply in terms of their name or chemical composition.

The geometry of a building and its structure can be described and communicated in a number of physical ways—by means of scale or schematic drawings, models, absolute or relative dimensions, absolute or relative locations of components. Drawings can be strictly two-dimensional or represent three dimensions in perspective or isometric views. Some drawings can show components; others their relative positions to assist manufacture and assembly. Equally important, though, and often much more effective and time-saving is the reference to precedent, to the knowledge of the builder. This knowledge can be basic, at the level of geometric concepts known to the builder, such as circles, squares, pyramids and so on; or at a higher level involving concepts such as arch, truss, beam or column. It can even be at the level of part or of whole buildings known to the builder ('I want a building like that one over there').

The precise means of providing a geometric description of a product can, therefore, depend greatly on the skills of the person making it—a mediaeval mason was undoubtedly more skilled at imagining the complex three-dimensional shapes of stones needed to build the roof vaults of cathedrals than his equivalent on a modern construction site. A consequence of modern design and construction processes has been the considerable deskilling of many site-based personnel. Until a century or so ago, there were few specialist designers. Design by an architect or even client was rather more schematic, with the construction engineer, builder or specialist manufacturer providing appropriate details.

The means of describing geometry will also depend upon the way in which the product is manufactured—complex three-dimensional shapes made by a casting process are still often better described in the form of a model rather than a set of drawings. Nowadays some products are manufactured using computer-controlled machines which receive their instructions direct from the computer on which the designer 'drew' the product at the design stage.

Ultimately, all ways of describing geometry are quantitative. The description of a building or a structure, forming part of the output of a design procedure, is a quantitative model of it and at various times in history, different types of mathematical model have been used—numeric, geometric, algebraic, trigonometric and vectorial. It follows that, if a geometric shape is to be communicated as a quantitative model, it must be possible to describe it mathematically. This simple fact has had a great influence on building design—it is far more convenient to describe a semi-circular arch than one which is more difficult to draw, such as a part of a parabola or cycloid. A curve or shape with no known equation can clearly only be communicated by means

of a sketch, template or model. In this particular case, the geometry is being communicated without the need for a mathematical model.

It is also important to notice here the focus on the *process* of drawing rather than on trigonometric equations—the fact that a parabola actually has a very simple equation in Cartesian geometry is of little consolation to the person who has been asked to construct a parabolic bridge arch. The lasting popularity, over many centuries, of the semi-circle and many-centred arches (comprising from two up to as many as thirteen circular arcs, see Fig. 13.11) compared to the ellipse, can be readily understood when one imagines the setting out problems an ellipse brings. Describing the geometry of a building is, ultimately, a process of communication and there has always been much to be gained by bearing in mind an effective means of geometrical description at the earliest stages of design.

5.1.2 Justification

A design for a proposed structure or building cannot simply be any design. In addition to satisfying functional and economic requirements, it will also be necessary to have confidence that the design will be structurally satisfactory—ultimately it must carry the loads it is expected to encounter without collapse or undue deflexion. It is absolutely necessary to be able to provide a convincing argument to justify a design to the many persons who have an interest in the matter—the future owners of a building or structure, the users, the owners of finance lent to carry out the project, the many persons employed during the construction and, not least, the designer(s) themselves. In the last resort, following a failure or collapse, a court of law will require evidence that the proposed design had been adequately justified.

The justification of a design has, over the centuries, been effected in many different ways. Indeed, at any one time, a number of different types of justification have been, and are, used. The justification must provide a reason or a number of reasons why faith can be put in a proposed design and to this end both deductive and inductive logic are used, usually in combination.

Deductive reasoning within the process of justification constitutes a chain of explanation which rests upon a series of assumptions or familiar and uncontroversial statements of fact or belief, and is built up by means of logical rules, nowadays usually the logical rules of a branch of mathematics. As such, so long as the assumptions are valid, deductive reasoning is infallible. Deductive reasoning constitutes an explanation and, since explanation is one of the functions discharged by scientific theories (see Appendix 2), it is not surprising that theories of science should play the important role they do in design procedures. This is the case both for modern theories (such as elasticity and strength of materials) and also for theories about the world based upon older beliefs involving notions of harmony, proportion and shape, drawn from the sciences of harmonics and geometry (geometry was considered a practical science until relatively recently). Explaining the behaviour of the world by means of the sciences and various branches of mathematics is not a recent phenomenon.

Inductive reasoning is based upon experience and precedent. Alone it can only provide a pseudo-logical justification for using a design solution which has been successfully used before—pseudo-logical because it rests upon a *ceteris paribus* clause and is therefore fallible in the case when other things, unexpectedly, are not equal.

While induction is fallible, the confidence with which an inductive justification is used increases with the number of times it has been successfully used before: induction is a very human process—convenient but fallible. Unlike deduction, induction does not constitute an explanation. Thus, data collected from experience and conveniently summarized in a simple algebraic formula or a geometric design method must be distinguished from deductive, explanatory justifications based upon theories about the behaviour of the world (both ancient and modern) which may also make use of algebraic or geometric techniques.

The justification of a design combines both induction and deduction (except when induction alone is used). While any deductive argument might in itself be infallible, each new application of that argument to a new design in new circumstances using a different batch of materials, can only be justified from experience using induction—the new use of the argument is judged to be 'the same as' a previous, successful application (it is, of course, never possible to be sure that a new application is 'the same as' a previous occasion).

Any deductive justification of a design must be proven inductively at three different levels. First at the level of the assumptions (e.g. that a material is Hookean, or that the notion of a pin-joint is valid). Secondly, at the level of the theory, which is that particular set of deductive reasonings based upon the stated assumptions (e.g. that a structure can be represented by a certain mathematical model). Thirdly, there remains the process by which the quantitative results of the mathematical logic of an explanatory justification are interpreted for application to the real world (e.g. the modification of calculations by empirical constants such as a factor of safety).†

In §5.1.1 we saw that it is usually not a good idea to conceive of a design which is difficult to describe or which cannot be conveniently described using simple mathematics: so too with the justification of a design. In general, it is wise to select designs which can easily be justified. With this advice in mind we can now see why certain types of structure have been popular, both in the past and in the present: they have been easy, or at least not impossible, to justify using means of justification available at the time. Thus in the 1860s and 1870s there was almost a craze for statically determinate roof and bridge trusses which could be justified using the recently developed graphical statics—Maxwell diagrams using Bow's notation. More recently, the shapes of concrete shell roofs are usually chosen with the means of justification in mind—the mathematics of shell theory appropriate to cylindrical shells, hyperbolic paraboloids or other shapes (such as the example of a printing works in Debden mentioned in §5.1.3).

This principle alone constitutes perhaps the most dominant way in which 'theory' can influence 'practice'. A designer is most likely to select particular structural forms and actions because of the ease with which they can be justified using the theory which is currently available. Conversely, research into new theories and the development of new design procedures to help a designer are only likely to occur when the will to construct a new form of structure outweighs the convenience of using forms which can already be described and justified using established theories and design procedures. It thus becomes almost second nature to designers only to suggest ideas

† This last level is analogous to the metaphysical aspects of the functions of scientific theories and their interpretation (see Appendix 2).

which can easily be justified, just as they tend also to suggest only those ideas which can be easily described and, of course, constructed. Although clearly there are good reasons for only conceiving of designs which are easy to describe, justify and construct, there is also the hidden danger of failing to be aware of the inherent conservatism it can lead to. Just as we are all prisoners of the mother tongue we speak so too are we of the structural language we know—but more of that in the next chapter.

5.1.3 The inter-relation of description and justification

The twin processes of description and justification in a design procedure have been treated separately to demonstrate the distinction between them, and in many design procedures they are, indeed, distinct. Often, however, the two processes proceed in close conjunction. A technique of description is conceived with more than half a mind on the technique of justification which will be required. A structural form is chosen and described in anticipation of the method and result of the deductive reasoning which will be used, be it in terms of statics and elasticity or in terms of a certain combination of circular arcs and proportions or ratios of certain dimensions (as in Gothic and Renaissance design procedures). At times it would seem that the processes of description and justification are fused into one.

Examples of anticipating a method of justification illustrate clearly the influence such methods have over the design procedures used at particular times and places throughout history—or, in conventional terminology, the influence of theory upon practice. The Renaissance practice of favouring the semi-circle and circular arcs was not only due to the fact that they were particularly easy to describe. The shape was closely associated with its many previous and successful uses—justification by induction (precedent). It was also closely bound up with the contemporary belief that the circle was one of the shapes used by God in creating the world *because* of its suitability to the purpose. Such reasoning gave deductive justification for the use of the circular arc. (We have since come to doubt the assumptions upon which this logic was based.)

A similar, and more recent example of the fusion of description and justification is to be found in the design of the concrete roof over the Bank of England printing works at Debden in Essex, by the firm of Ove Arup and Partners. It was required that the roof be arch-shaped but asymmetrical to enable the provision of adequate lighting and headroom. It was also desirable that the shape be a single curve rather than, for instance, a series of circular arcs, in order to facilitate the setting out of the formwork and the checking of the finished structure. For the purposes of justifying the design, a shape was required which had an equation particularly suited to the preferred type of mathematical model and method of structural analysis, which involved the shape parameters raised to the fourth power. After a bit of a search, it was found that the equation of one of family of curves, known as the Lemniscate of Bernoulli, simultaneously satisfied all the requirements (Fig. 5.3).

Further examples could be given, drawing upon the many geometrical shapes which are found to be contained in the mathematical analysis of engineering structures, e.g. the straight line, the triangle, the circle, the parabola, the catenary, the hyperbolic paraboloid. This observation of a close correlation between the geometry of a structure and its structural behaviour is rather more significant than might at first appear. The behaviour of a structure under a given set of loads depends upon only

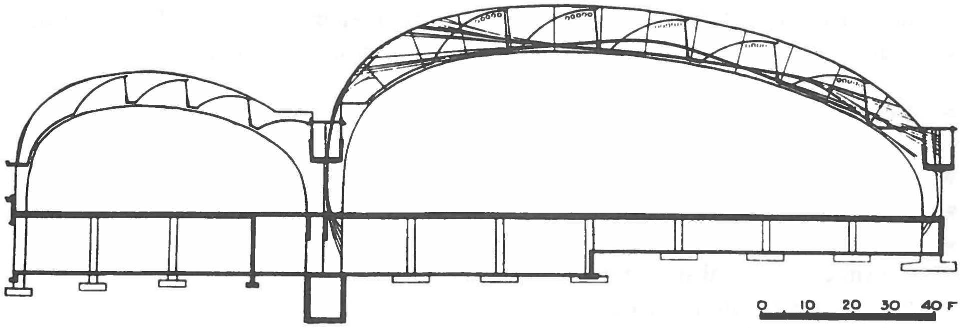


Fig. 5.3—Cross-section of a roof having the shape of a Lemniscate of Bernoulli. Bank of England printing works, Debden. Engineers: Ove Arup and Partners. [Robertson *et al.* 1956]

two things—the material(s) of which it is made, and the disposition of this material in space. The stiffness and strength of a material are independent of shape and geometry, indeed, they were deliberately so conceived. Ultimately, then, the manner in which a structure behaves (though not its amount) depends only on the quantity of material in a component (its cross-sectional area), the disposition of this material across the section (the second moment of area, I), and the way in which separate components are located relative to one another in space (how they are connected). All of these are dependent only upon geometry. There is, then, in the case of structures, a rather special inter-relation between the description and justification which one would not find in the case of designing heat engines or electrical machines. This is of immediate practical consequence to those persons seeking to tie up the computer-aided drawing of structures with the justification of the designs using structural analysis—but we are jumping ahead again.

5.2 INPUT—ENGINEERING KNOWLEDGE

Having considered the output of the design procedure, we should now consider the input. This includes the full spread of 'resources' from which a designer can select appropriately to form the input. Ultimately, there could be the whole of human knowledge and experience, but that is perhaps a little too wide, for we are each individuals living at only one time in history and having only limited engineering knowledge and experience. Within this phrase, however, we must include all of what an 18th century person might have meant by the science of engineering—the complete body of knowledge pertaining to engineering, not just the mathematics and modern scientific theories of, for instance, bending and shell action. In addition to the, what might be called, 'public' knowledge which can be stored up cumulatively and passed on from generation to generation, there is also personal knowledge—knowledge gained directly by experience which is virtually impossible to pass on to anyone else.†

† For further discussions of knowledge, see Chapter 6 and also [Hall 1978; Layton 1974; Polanyi 1958].

The knowledge and experience which an engineer might use while designing a structure could be drawn from a wide range of sources, for instance:

- physical and engineering data
- physical and engineering laws
- physical and engineering theories
- physical and engineering rules
- mathematics (geometry, algebra, statics, etc.)
- experiments on real structures
- experiments on model structures
- analogue models
- mathematical models
- personal experience
- collective experience
- historical precedent
- engineering 'know-how'
- intuitive knowledge of structural action and behaviour
- qualitative understanding of structural action and behaviour
- concepts in the language of engineering
- codes of practice

The list could easily be extended. More important to the engineer than the *amount* of knowledge, though, is the selection of appropriate bits of knowledge and how the engineer should use them to greatest effect. This discussion will continue in §5.4 and the next chapter.

5.3 REGULATION

Before considering what the engineer does in creating and following through a design procedure, a few words need to be said on factors which influence him in this process. Some of the factors included below might appear to be more than mere influences—the client, architect and the economic factors might often seem more like severe constraints. However, in the present discussion concerning the process of design, they are only influences which provide the boundaries within which the designer designs—they do not have the same effect upon a designer as, say, the law of gravity or an engineer's feel for the stiffness of a tube. The various factors which regulate and influence the design procedure might include:

- the client's design brief
- the architect's design
- criteria of structural adequacy
- non-structural criteria of adequacy
- relevant codes of design practice
- accepted practice in the profession
- an engineer's education
- the current 'state of the art'
- available methods of construction

- availability of materials
- professional demarcation
- contractual relationships with other professionals
- economic factors such as costs and time
- building regulations
- the law, etc.

Factors such as these have been collectively and succinctly referred to (in the context of structural safety) as the 'engineering climatology' at a particular time and place. [Pugsley 1972].

5.4 THE DESIGN PROCEDURE

The notion of the design procedure is not intended to encapsulate or describe the very act of creation by a design engineer at work. The design procedure can be thought of as beginning with or being triggered off by an act of creation—the conception, so to speak (the French use this very word). Precisely how and why a structural engineer chooses or conceives a particular structure for a particular purpose is a process, if one can call it that, so nebulous and individual that I doubt if it is possible to study it at all. The process varies from person to person, and project to project. Sometimes it might be a flash of inspiration; at others it might be a choice following the detailed investigation of several different options; on other occasions it may be a gradual refinement of a single basic idea.

Such acts of creation in structural engineering probably have a great deal in common with acts of creation, or conception, in other creative fields such as music and architecture. Composers and structural engineers alike have great difficulty in trying to express how they came to compose and invent their melodies and harmony, or conceive their structural designs. For another person to try to study this process from outside is even less likely to discover anything. One can only surmise how the ideas for new structures originate. Ultimately, the 'springs of structural invention', as they have been called [Mainstone 1973], consist only in our experience and memory and various ways of drawing upon these. In fact, the structural building blocks—the beam, the arch, the tie, the shell and so on, are very few in number, about the same number as the notes of the musical scale. They can, however, be combined in almost an infinity of ways.

At the conception stage of a proposed structure, even the most fleeting idea is appraised and 'tested' in a split second—the way it would carry the loads imposed on it and deflect, its likely suitability to purpose, its appearance, various consequences concerning type of material, time and cost of construction and so on. Such processes are the very essence of what it is to have experience and use it—just as most people can 'see' five or six objects on a table with having to count them—the process is almost unconscious. At this early stage it is often possible simply to *know* that an idea will work in principle and to know that it will be possible to develop the idea and fully work out the details.

However, sooner or later during the development of a design, it will be necessary to formalize the process, to describe the structure using linguistic concepts, to

describe it in ways which will enable other people to grasp what the idea is and consciously to work out consequences of a certain choice which are beyond the capacity of a human brain alone. It is at this point, which will vary from person to person and project to project, that the more rigorous stages of developing a design start. Here begins the main body of the design procedure.

The design procedure for a structure is the series of steps which need to be taken in order to develop an idea for a proposed design from its conception to the production of its formal description and justification. For a type of structure which has been designed and built before, the design procedure will probably be familiar to the engineer in most of its detail. For a new type of structure, however, not only will the engineer face the task of creating a new design, but will have to develop a new design procedure as well. Indeed, this is likely to be a major part of the work and involve just as much creativity as the conception itself.

The design of the Sydney Opera House is a well-known recent example. The original conception by the architect was of several pairs of thin concrete shells leaning against one another. Unfortunately, this idea is physically impossible on a large scale and it took a very great deal of engineering design activity to make the original scheme work. Some of this activity was the proposing of alternatives to the original conception such as different ways of stiffening the shells and different shapes of the shell surfaces. However, most of the work was directed at developing ways of justifying the various alternative schemes, by means of testing small-scale models and developing the use of computers to tackle mathematics of unprecedented complexity. The development and testing of new design procedures was perhaps the greatest achievement of this well-known project.

In a sense, a design procedure begins as soon as one starts to talk about a structure which has been conceived. As we shall see in the next chapter, how we imagine and talk about a structure constitutes the very most fundamental way in which we begin to create a model of a structure, the loads it will encounter and the materials of which it will be made. When facing the task of designing a new type of structure, an engineer might realize, for example, that a new means of defining the desired geometry will be needed, or that the mathematical tools at his disposal will not provide an adequate justification of the proposed design. In one sense, this situation makes the engineer's job easier than a composer of music, since the engineer often knows what sort of a new design procedure to develop. On the other hand, it may be (and often has been) the case that an engineer believed that a certain design procedure would give an adequate justification of a proposed design, but later found out that it did not—the design of the Tacoma Narrows suspension bridge in 1940 is a case in point (see §13.4).

Two important consequences of the notion of the design procedure are that it is possible to produce very similar structural designs using different design procedures and that similar design procedures can lead to significantly different structures—there is no logical connection between the two. Major differences between designs are most likely to arise from different basic conceptions and from the many outside influences (client, architect, etc.) not as a result of the design procedure itself.

It might seem from the use of the word 'procedure' that freedom of choice, originality and creativity are precluded. This is not in any way intended. The previous paragraphs have how creativity occurs at the very earliest stages of a design, at its conception. Yet

even after conception, during the subsequent design procedure, it is still possible to be creative and original. Borrowing from Wittgenstein's idea of the 'language game', design too can perhaps be viewed as a game, having an overall goal or aim, yet moderated by the rules of that game. The originality comes in choosing how to achieve the ends with the means available while subject to the restrictions of the regulating factors. As such there need be no less freedom, nor limitations to creativity and originality than are available to a composer of music 'restricted' by the twelve notes of the chromatic scale and the instruments of the symphony orchestra. The word procedure is chosen to reflect the idea of 'a particular action or mode of action' or 'course of an action' [SOED]. In this way a descriptive notion of how to do something is implied, without there being a rigid set of rules.

The idea of the design procedure, as a statement of how a designer could or should go about designing particular structures is not new. It is to be found in several classic texts which set down design procedures varying in scope from whole cities down to individual pinnacles on a cathedral:

- cities, temples, theatres, weapons [Vitruvius c. 25 BC]
- buildings, bridges [Alberti 1485]
- various parts of cathedrals [Roriczer 1486; Lechler 1516]
- timber arch and truss bridges [Wiebeking 1810:124ff]

Recently, the idea of the design procedure has been gaining in popularity.† However, it is seldom developed in any detail, often being presented as a rought outline only. Nor is it emphasized that it is one of the main skills of a structural designer to *devise* design procedures rather than simply to follow them.

Flow diagrams can provide an effective way of summarizing the essence of a design procedure and it is unfortunate that they are not more widely used. The example given in Fig. 5.4, and the larger ones in Appendix 3, contain an enormous amount of engineering knowledge representing the work of many thousands of man-days and, of course referring to much, much more. Unlike what one usually finds in engineering books and many codes of practice, they give a clear indication of how to select and to use knowledge and information. Their value in storing knowledge and enabling it to be communicated to professionals or students can hardly be over-estimated. As with calculations and computer programmes, however, one must always be kept aware of the type of structure and circumstances for which the design procedure was created.

It is worth noting here that published examples of design procedures are rather rarer than might be expected or hoped for, compared to the plethora of material devoted to engineering science. One reason is perhaps that designers are engaged in a process of performing a skill which, like swimming and playing a musical instrument, is very difficult to describe. There is also perhaps a certain reluctance which practising engineers have to writing down their thoughts and methods, a phenomenon which has been called the engineer's 'papyrophobia' [Price 1965].

† See, for example [Cross 1935b:97; Mainstone 1959:214; Gregory 1963; Preece & Davies 1964:13; I.S.E. 1969:9; Report 1971:40; Sibly & Walker 1977:197; Kong & Evans 1980:314; Curtin *et al.* 1982; Grant 1982; BS 8110 1985: Part 1:3/1].

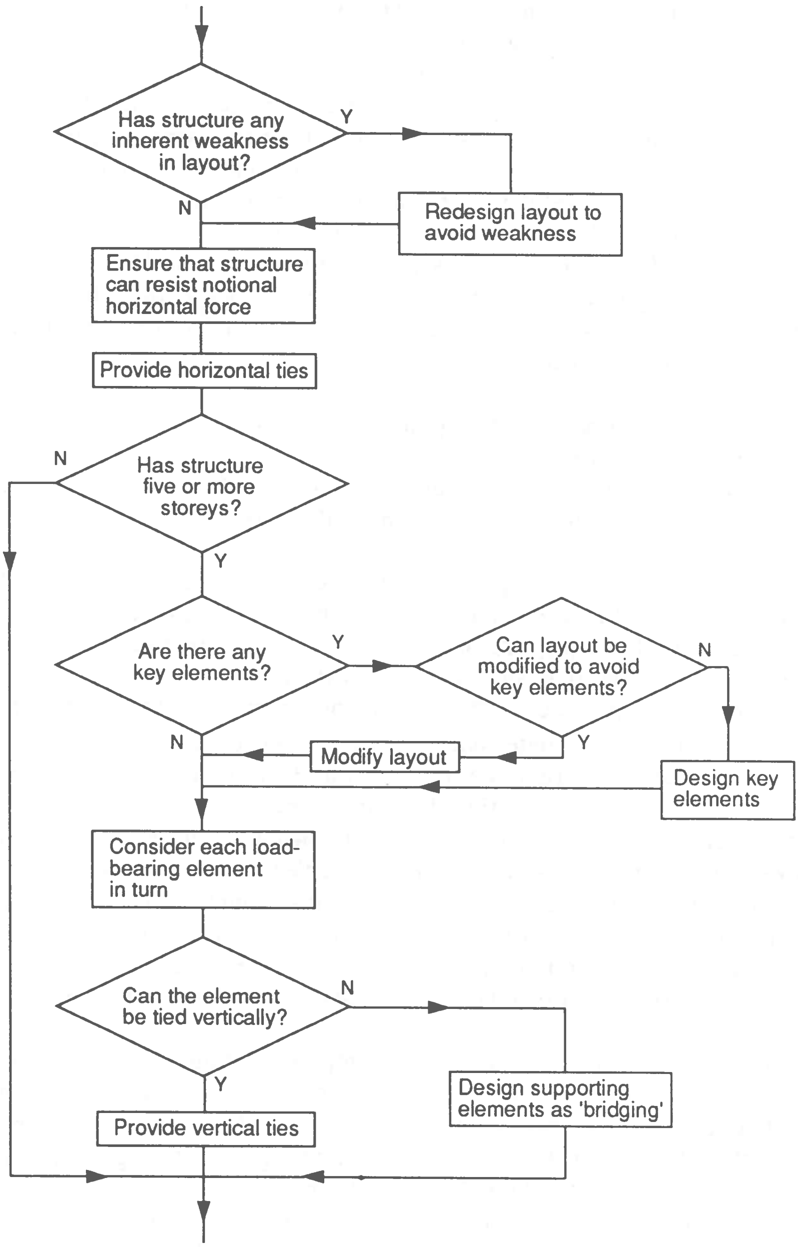


Fig. 5.4—Flow diagram summarizing part of the design procedure envisaged by the Code of Practice for 'ensuring robustness' of a reinforced concrete building.
[BS 8110: Part 1, Section 3]

One of the rare occasions when design procedures are scrutinized and publicized is during the inquiries which follow major structural accidents. Two recent examples are the collapse of the steel box-girder bridges at West Gate Point in Australia, and at Milford Haven in Wales both in 1970 [Report 1971; Report 1973]. Both inquiries called upon the structural engineers to justify both the bridges' designs and their own design procedures to a court of law. In the court's opinions, they failed adequately to do so, although no blame was apportioned as they were following accepted practice. The positive outcome, particularly of the Welsh accident, was an extensive appendix to the report of the inquiry which set down a new design procedure for steel box-girder bridges and this became the basis of a new Code of Practice (BS 5400). In fact, there has recently been a wider trend for the Codes of Practice to be seen as a statement of design procedure [I.S.E. 1969; BS 5950 1985; BS 8110 1985]. At present, however, there is often still a lack of clarity as to the precise role of the design procedure.

One proposal that has come near to encapsulating the notion of the design procedure presented here, incorporating the twin functions of description and justification, is the attempt to distinguish between 'computations' and 'calculations' in structural design [I.S.E. 1976]:

> The term computation refers to the processing of numerical information to arrive at an answer. This can be done on the back of an envelope or in a large computer, and in neither case are the reasons for the calculation or the significance of the answer recorded. The formal calculations prepared by structural engineers are much more than computations. They are intended to justify and explain the decisions that led to the choice of structural type, member size or joint detail. In these calculations the numerical work is often secondary to the explanations. [I.S.E. 1976:24]

Other authors have used a variety of other terms signifying similar (possibly identical) notions to that of the design procedure; these include design, design-method, -methodology, -process, -theory, -philosophy, -technique, -rule, and calculation procedure model. However, a formalization of these concepts has generally been lacking and they have not been used to develop a deeper understanding of the nature of design and its historical development (exceptions are [Blockley 1980; McCrory 1974]).

5.5 CONCLUSION

The notion of the design procedure has a number of particular advantages over the conventional concepts of theory and practice, and helps to avoid many of the problems concerning an understanding of the nature of engineering which were outlined in Chapter 1:

- it focuses the attention firmly on the activity of design in engineering;
- equally applicable to all times of history
- knowledges the influence of all types of engineering knowledge upon the design of engineering structures (not only theory);
- identifies a new 'parameter' which can play a role in the activity of engineering design similar to the role which the scientific theory or hypothesis plays in the field of science.

The design procedure has been shown to have two principal functions—to describe and to justify a proposed design. It is able to discharge these functions by means of its empirical and metaphysical aspects, in ways analogous to the ways in which scientific theories are able to discharge their functions (see Appendix 2).

Before proceeding now to discuss in more detail the input side of the black box model of the design procedure (engineering knowledge), it is worth glancing back at what Vitruvius wrote on the subject of design some 2000 years ago:

> Ratiocinatio . . . est, quae res fabricatas sollertiae ac rationis proportione demonstrare atque explicare potest.

In the light of the present discussion, it would seem to make more sense of this passage than other translations (see Appendix 1) to translate it as:

> Designing is the ability to describe and to justify the design of buildings by means of the rational use of engineering knowledge and scientific principles [Addis].

6

Engineering knowledge

In their concern with both new and existing engineering structures, designers draw upon many types of knowledge both for the purposes of describing and justifying a design and, more generally, in their understanding and conception of the behaviour of structures. This and the next chapter will deal with these matters by considering a number of these quantitative and qualitative aspects of structural engineering knowledge.

Knowledge related to the quantitative aspects of engineering is itself of several kinds: empirical, explanatory, or mathematical. This classification will enable the means by which design procedures are able to discharge their functions to be better understood. There is here, not surprisingly, a similarity with the epistemology of scientific theories and the ways in which they are able to discharge their several functions (see Appendix 2).

6.1 EMPIRICAL KNOWLEDGE

Empirical knowledge is gathered from experience and experiments in the real world. Empirical knowledge can be qualitative and quantitative. Generally speaking, qualitative knowledge precedes the quantitative, especially since the latter depends also upon knowledge of one or another branch of mathematics. Strictly speaking, any quantitative empirical knowledge depends for its meaning or interpretation upon the existence of a theory about the world; this will be discussed in several later chapters. Assuming, for the moment, an interpretation and understanding of the information, empirical knowledge can be of several kinds.

6.1.1 Empirical data

Empirical data represent the most basic form of knowledge which can be derived from experience of the real world. Each datum (atomic fact) is, strictly, unique and isolated since it cannot be related, *a priori*, to any other datum. Thus, properties of materials and of artefacts made of these materials are, when taken in isolation,

simply facts and can hardly be considered as 'properties' at all. As such, empirical data alone have very little significance. Their utility is enhanced when they form the basis of various generalizations.

There is one rather special example of a piece of empirical data which does almost have a life of its own—the empirical constant. This is a device developed at least as early as the late 18th century to allow general laws to be modified by an appropriate amount to suit certain particular circumstances. A typical example is the factor by which the general formula relating the maximum load a beam can carry to its dimensions is modified to suit different materials and shapes of a beams. It was well known long before the development of quantitative theories of bending that the strength of a rectangular beam varies with its length and breadth and the square of its depth. This law was made useful to designers by the simple application of a numerical factor (an empirical constant) the value of which depended upon the material. Empirical constants are still widely used by engineering designers, especially in those areas of design for which appropriate scientific theories are inadequate or too complex for day-to-day use, and perhaps the most common of these is the 'safety factor' which will be discussed later.

6.1.2 Empirical laws

Humans have an innate propensity to generalize by means of inductive logic and to group individual data according to various ways of seeing the world. Thus, a series of observations concerning wood or planks made of wood, are deemed to be 'similar' in a relevant way and lead to general statements about, say, the strength and stiffness of wood, or of planks made of wood. It is here important to be aware of the frequently misleading use of phrases incorporating the idea of 'similarity' or 'the same as'. We do not identify similarity and then categorize accordingly. Rather we learn to categorize in ways which are useful or productive, and then pronounce the grouped individua as similar.†

An empirical law constitutes a grouping of data. This may be collected through random experiences and observations or by means of a series of deliberate tests designed to generate grouped data and thus yield a useful law, although strictly speaking, it is not possible to discover a law without first having a hypothesis that one might exist and is 'awaiting discovery'. Empirical laws have existed for thousands of years. Indeed, they are fundamental to our experience of the world and our use of language. For most of history the data have not been quantitative in the manner to which we are nowadays accustomed; however, that is not to say that the ideas of magnitude and measure are absent from, say, the laws relating to the structural properties of timber and stone given by Vitruvius. It is inconceivable that the strengths and stiffnesses of beams made of stone or timber of different dimensions were not well appreciated and accurately known at least as far back as Greek times. For many reasons, such as a lack of need, suitable means of expression or perceived benefit, quantitative structural laws were not recorded until much later. However, we do know that the Greeks did have engineering laws for the performance of stone-throwing weapons and used

† The use of engineering concepts such as 'arch' and 'truss' will be discussed in Chapter 7 in this light. For a fuller treatment of the issue, see [Wittgenstein 1958].

them in their design [Marsden 1971]; they also used formulae in the design of buildings, though not obviously incorporating what we would now term structural criteria (see §12.2).

It was only with the birth of the modern methodical approach to experimentation, following the work of Francis Bacon (around 1600), that laws take on their contemporary forms. Between 1662 and 1664 at the Royal Society, Robert Hooke and others carried out a large number of tests to establish the breaking strengths of wires (in tension) and beams (in bending). To the modern eye there is apparent what can only be called a remarkable degree of randomness in the way the materials, dimensions and even temperature and air pressure were varied in the hope of discovering an empirical law [Dorn 1970:245–255].

It is in the nature of empirical laws to be observed first in a particular case and later to be generalized (by inductive logic) to apply to cases not so far tested. Later work by Hooke concentrated first upon wood and metal and then upon 'stones, baked earth, hair, horns, silk, bones, sinews, glass and the like' and led him to suggest his famous law *'ut tensio sic vis'* applicable to 'every springing body' [Timoshenko 1953:20].

Qualitative empirical laws relating to structural engineering include many which are common knowledge (and have been for millenia) and yet contain the germs of sophisticated engineering ideas, for example:

- a plank bridge is at its most vulnerable when the load it supports is in the centre;
- the deflexion of a plank bridge increases with load and the length of span and decreases with increasing depth and width of the plank; the deflexion also depends upon the material of which it is made;
- a wall one brick thick is easier to overturn than one which is two or three bricks thick;
- long slender timber props are prone to buckling; short ones are not.

More sophisticated empirical laws were often observed by craftsmen and builders:

- semi-circular arches made of thin voussoirs were not as strong or stable as one made of deep voussoirs;
- diagonals in bays near the ends of a cross-braced truss bridge suffer greater compression than those in the centre;
- two planks make a stronger and stiffer beam if they are joined together by glue or pegs to prevent them sliding relative to one another.

Empirical laws of the quantitative kind had already reached great sophistication and usefulness at least as early as the 1730s. Musschenbroek published many results of tests on beams and columns of many types of material and presented the results as generalized laws, dependent on the dimensions, raised to the correct powers, and modified only by empirical constants according to the material. Thus the data behind Euler's famous buckling formulae for columns were known at least 50 years before he worked on the problem.†

†Euler was not an engineer and his great achievement was to use the newly invented differential calculus to relate the buckling load rationally to the dimensions of the column and the stiffness of the structure.

Empirical laws serve, as do the empirical aspects of scientific theories (see Appendix 2), to store data about the world and to summarize it. They also, in some measure, allow prediction—by generalizing both beyond the range of tests carried out and to other new circumstances supposed to be similar in relevant ways. The dangers lying therein are serious, as Sir John Herschel noted:

> When empirical laws are unduly relied on beyond the limits of the observations from which they were deduced, there is no more fertile source of fatal mistakes. [Herschel 1830:179]

Empirical laws, be they qualitative or quantitative, are not, however, explanatory. Their role in design procedures is one of providing justification of a design only by direct reference to, or comparison with actual precedent, or to precedent which is believed to be sufficiently similar in relevant ways. Justification by appeal to precedent is inductive not deductive.

6.1.3 Empirical rules

Empirical data and laws are, in themselves, of no direct use in the process of description in a design procedure since they contain no prescription for human action. Designers need rules:

> principle[s] regulating the procedure or method necessary to be observed in the pursuit or study of some art or science. [SOED]

A designer can only make use of the information contained in an empirical law if there also exists (or could exist) a corresponding rule which indicates that following a certain course of action will lead to a certain result. A rule can thus be the converse of the process by which data are gathered together to yield a law. The empirical law is used within a rule to generate data as part of the process of description within a design procedure.

Neither the knowledge of, nor the knowledge of how to use, an empirical rule, need be a conscious process. (The unconscious use of rules as part of the qualitative and intuitive approach to design will be treated further in Chapter 7.) Thus:

> The aim of a skilful performance [can be] achieved by the observance of a set of rules which are not known as such to the person who follows them . . . Rules of art can be useful, but they do not determine the practice of an art; they are maxims, which can serve as a guide to an art only if they can be integrated into the practical knowledge of the art. They cannot replace the knowledge. [Polanyi 1958:49-50]

Empirical rules have often been referred to as 'rules of thumb' and, in this more familiar phrase, are associated with approximate results and with the days before 'scientific design'. Yet present day Codes of Practice still contain many rules for the use of empirical data based solely on the accumulated experience of the civil and structural engineering industry concerning what is known to work. Examples include recommended working stresses, factors of safety, the use of particular mathematical models of structures and the many empirical constants (such as the end-fixity of beams and columns) which appear in conjunction with mathematical expressions to modify theory for use in practice.

Empirical rules are not explanatory. Their purpose is different from both empirical data and laws, and explanatory theories. Rules prescribe a certain course of action. While data, laws and theories can be more or less correct, rules can only be more or less effective. As distinct from empirical rules, one philosopher has introduced the idea of a 'grounded rule' which, in the language of the present discussion, is one which is based upon an explanatory theory of some kind, and so can also serve in the process of justfication as well as description (see [Bunge 1966] for a fuller discussion of the distinctions).

The prescriptive function of rules is perhaps more clearly brought out in the phrase 'design rule'. It contains not only the idea of 'a principle regulating the procedure' [SOED] but also the purpose for which the rule is intended. Like 'empirical rule' the phrase is commonly used to refer to times before the supposed first use of theory and, in particular, to imply design methods based upon criteria different from those which are nowadays current—such as architectural or geometric criteria rather than those based upon statics, elasticity and theories of strength of materials (see, for instance, [Mainstone 1959 and 1968]).

Design rules are by no means, however, things of the past. The activity of design cannot proceed without prescriptions for action—in other words, rules. Examples of design rules will be elaborated in later chapters where they are presented in terms of the design procedure which, it is proposed, is a more appropriate and less pejorative phrase.

6.2 EXPLANATORY THEORIES

A theory, as distinct from theory (without the article), is

> a scheme or system of ideas or statements held as an explanation or account of a group of facts or phenomena. [SOED]

Modern scientific theories of engineering and physics are familiar enough to need little more than a mention here.† A typical example is provided by theories of bending. Galileo was the first (1638) to try to relate the strength of a beam loaded in bending, to its dimensions and the strength of the material in simple tension. More sophisticated theories were developed later both to account for the observed deflexion of beams and to be able to quantify the supposed stress at any point on the surface of or inside a beam.

Such theories were, and are, primarily the concern of persons we nowadays call engineering scientists, and their methods and their aims were of the same types as those followed by physicists in their quest to comprehend nature—astronomy, heat, light, the structure of matter etc. As history has progressed, new scientific theories have been developed and often supplanted their predecessors in being more general, or more powerful or in taking the explanation of nature to more fundamental levels. Scientific theories are, however, not of direct use to engineering designers. Theories

† See Appendix 2 for a summary of their functions and formalism, and also [Braithwaite 1953].

make their contribution rather by deepening the qualitative understanding of nature or by being incorporated into design procedures as part of the processes of description and justification, for instance by providing justification of a grounded rule.

The scientific theories of the past three centuries (post-Baconian) were not, however, the first attempts at understanding and explaining nature. From Greek times, and probably earlier, right up to the 17th century, there had existed many types of theory with this aim. Yet often they were founded upon utterly different assumptions and different concepts in order to achieve the same principal goal. As will be illustrated in Chapter 12, pre-Baconian theories, like their later successors, were also employed in design procedures in both description and justification.

The most important concept from pre-Baconian times was 'harmony'. Harmonics was one of the most important areas of study for Greek philosophers and is an early example of an experimental approach to understanding nature:

> Tradition ascribes to Pythagoras (c. 570–497 BC) the discovery of the fact that small number ratios (namely 2:1, 3:2, 4:3) correspond to the most consonant musical intervals: octave, fifth, and fourth respectively . . . Whether this discovery inspired the Pythagoreans to more intensive speculations on number or only strengthened a tendency which was already present, it is a fact that everywhere they began to see numbers and numerical ratios . . . This 'everywhere' is to be taken absolutely literally. In astronomy numerical ratios were supposed to exist between the distances of the various heavenly bodies from the centre of the world, and since these ratios also meant musical intervals, these intervals came to be regarded as being present in the celestial spheres. From this sprang the wildly speculative doctrine of the harmony of the spheres which for ages was again and again to fascinate people of a mystical turn of mind . . . Aristotle (384-322 BC) gave two succinct formulations of this Pythagorean way of thinking: things are numbers; the entire firmament is harmony and number. [Forbes & Dijksterhuis 1963:33-34]

Not only did the subject of Harmonics attract the attention of other Greek thinkers such as Euclid, Archimedes and Ptolemy, it persisted, with some interruptions, through the Renaissance right up to the early 18th century [Stevens 1990]. Galileo, Kepler, Descartes, and Newton were only some of the many 17th century figures who moved easily, and often consequentially, from mathematics to astronomy, harmonics, statics, optics and to the study of motion (see [Kuhn 1977:36–40; Wittkower 1952:124]).

Also amongst explanatory pre-Baconian theories, geometry must be mentioned. It is nowadays usual to treat this subject as a branch of mathematics. However, such was the perceived power of the deductive logic of geometry, and its intimate relationship with harmonics and small-number ratios, that it cannot be treated as 'merely' mathematical in earlier periods of history. The ratios associated with harmonics were usually depicted and conceived of as ratios of line lengths rather than of numbers or algebraic symbols, and simple geometric figures provided a means of encapsulating the ratios and proportions. Many examples of the role of the circle in the creation of the universe were found in the study of astronomy.

By mediaeval times the theories of harmonics had been further generalized, and hence made more powerful, by seeing geometry as yet another worldly manifestation (alongside music) of the simple ratios and relationships which were believed to have been used by God in creating the universe.

The philosopher Robert Grosseteste (c. 1175–1253) was perhaps the first to outline what is now usually called the hypothetico-deductive method for science, and the principle of falsfication, whereby a hypothesis should be rejected if it led to conclusions found to be at variance with experience. He also declared that:

> without geometry it is impossible to understand nature, since all forms of natural bodies are in essence geometrical and can be reduced to lines, angles and regular figures. [von Simson 1956:198]

It was Saint Augustine (354–430) and Boethius (480–525) (both were philosophers and hermeneuts) who provided the means whereby the work of the Greek philosophers was incorporated into Christian beliefs. Their works on the science of music, mathematics and architecture sought to demonstrate the underlying principles, based upon harmony and geometry, of the world as created by God. Further manifestations of these principles were, of course, to be found in the Bible. Augustine took the Biblical passage *'Omnia in mensura et numero et pondere disposuisti'* (Thou hast ordered all things in measure, number and weight) and applied Pythagorean and neo-Platonic methods to the interpretation of the Christian universe, its creation and its order (see [von Simson 1952:10]). Concerning the design of buildings, the Bible also provided dimensional details of a number of significant structures—the Ark, Moses' Tabernacle, Solomon's Temple and the Celestial Temple revealed to Ezekiel in a vision. A well-known 14th century masonic poem even claims that:

> Solomon actually 'taught' architecture in a manner 'but little different from that used today' and that this science was directly transmitted to France. [von Simson 1956:33,38]

The writings of Augustine and Boethius dominated the Middle Ages, and the 'cosmic applicability of the laws of harmony' figures boldly in writings about both music and building throughout the Gothic period [von Simson 1956:125,191].

At the beginning of the Italian Renaissance, Alberti helped to revive the Greek and Roman influence on building by incorporating many of the ideas of Vitruvius in his *De Re Aedificatoria* of 1485. Both here and in the many other Renaissance treatises, the ideas of proportion, harmony, geometry and simple ratios were seen as a basis for building design:

> For Alberti—who follows here a tradition unbroken from classical times—music and geometry are fundamentally one and the same; that music is geometry tanslated into sound, and that in music the very same harmonies are audible which inform the geometry of the building . . . Alberti discuss[es] the correspondence of musical intervals and architectural proportions. With reference to Pythagoras he state[s] that 'the numbers by means of which the agreement of sounds affects our ears with delight are the very same which please our eyes and our minds', and this doctrine remains fundamental to the whole Renaissance conception of proportion. Alberti continues: 'We shall therefore borrow all our rules for harmonic relations from the musicians to whom this sort of numbers is extremely well known, and from those particular things wherein Nature shows herself most excellent and complete' . . . The architect who relies on those harmonies is not translating musical ratios into architecture, but is making use of a universal harmony apparent in music. [Wittkower 1952:8,97]

In summary, there is considerable evidence to suggest that, for at least the last 2500 years there have been theories providing an explanation of the world and that these were used to reinforce arguments in much the same way that we nowadays appeal to the laws of gravity and of mechanics. Whilst philosophers and scientists have now discarded many of the earlier theories in favour of 'better' ones, this fact does not alter the use made of explanatory theories in past eras.

There is, of course, little direct evidence for the use of logical explanations and justifications during the design of buildings from one or two thousand years ago. We do, however, know that geometry was used and that, then, geometry had powers of explanation which it has since lost (and which have been transferred to statics and theories of strength of materials, *inter alia*). We know of at least one geometry textbook of the 13th century which gave one of the functions of geometry as 'to give reasons' in the process of building, although, unfortunately, not which reasons (see §12.3). It should also be noted that there are many engineering structures built during the last century for which there is also no direct evidence that logical explanations and justifications were used—we nevertheless assume they were.

6.3 MATHEMATICS

While the primary function of theories is to explain the world, such an explanation remains qualitative unless there be an appropriate means by which the concepts of a theory can be made amenable to quantitative treatment. To be useful, a theory must have a mathematical formalism in addition to the metaphysical formalism (see Appendix 2). Such is the role of the many branches of mathematics which are used in the formulation of theories. The modern tendency is more and more towards using the phrase 'mathematical model' for any quantitative aspect of a scientific theory—mathematical because it is quantitative; model because it is a representation of the real world.

Each branch of mathematics which may be used in constructing a model comprises a set of *axioms* and *definitions* and a set of logical *rules of inference*. By applying a rule of inference to one or more axioms, a *theorem* can be obtained (or 'proved'); thus is created a statement containing new knowledge. By further applications of the rules of inference to combinations of axioms and already proven theorems, a whole logical system is built up dependent only upon a particular set of axioms, definitions and rules of inference. Such a system is not open to doubt; nor is there any question of testing it by means of experiment. Algebra, arithmetic, geometry and statics are all examples of such logical systems.

This view of mathematics is a comparatively modern one. Until the early 19th century no distinction was made between interpreted and uninterpreted logical systems [Hospers 1967:195]. It was always tacitly assumed that the axioms and definitions of a logical system of the kind described above were contingently true; that is to say, they had been, or could be tested experimentally and found to be true. It was by this means that they had come to be incorporated in the design procedures used by designers as well as philosophers. Such beliefs help to explain how it was that geometry could be believed to be of such universal application—from astronomy to music to the design of buildings. The theorems of geometry were treated as laws of

nature and assumed to have been verified by empirical testing and were thus believed to have wide-reaching powers of explanation. They were not seen as logical systems forming a branch of mathematics which is necessarily true. It is, therefore, likely to be misleading for us to presume that when a Greek or Gothic building designer was using geometry to plan (design) and even to set out his building, he was perceiving the process as we would nowadays.

Since the early 19th century it has been widely understood that the use of geometry, algebra and trigonometry does not, in itself, constitute a valid explanation or justification of a hypothesis or a proposed design for a structure. However, the same cannot yet be said of statics. There is still a tendency for people to see statics not as a branch of mathematics but as a branch of the natural sciences, and to believe that using statics must lead to confident explanations and justifications about structural behaviour, irrespective of the structure and the type and sophistication of the mathematical model used. One can see a parallel between what we now regard as the inappropriate use of geometry in the past and the more recent, sometimes blind application of statics to the design of buildings. This view is strongly reinforced during the education of engineers: after all, did they not do experiments as students to verify statics by measuring the deflexions of beams, and the forces in trusses? Did they not then write conclusions to their laboratory reports explaining that the differences between their results and the theoretical ('correct') answers were due to the sub-standard supports, poor-quality joints, inaccurate weights and inferior measuring techniques—any excuse rather than suggest that the simple mathematical model was a poor representation of a real structure (that the theory was wrong or, at least, inadequate)?

In the next chapter we shall continue this discussion by focusing on mathematical models, and the concepts upon which they are founded, and how they facilitate thinking about structures and their behaviour, as well as being used to explain and to calculate.

7

Thinking about structures

7.1 QUALITATIVE ASPECTS OF STRUCTURAL ENGINEERING

The previous chapter focused on the quantitative and formalized aspects of engineering knowledge—those which can readily be expressed in words, numbers and symbols, and combinations of these, which are of use in design procedures and the description and justification of proposed designs. However, in addition to these formal techniques, we also make use of the concepts behind the words and symbols at a more fundamental level, to help us think about structures and most of this thinking is undertaken not in quantitative terms, but qualitatively. In fact, such qualitative thought, being generalized, precedes the particular cases which the use of number and quantity define [Skolimowski 1966].

To think qualitatively about the behaviour of a structure is to use one's imagination and to use it no less creatively than a musician or artist producing ideas out of her head. In addition to the imagining of a geometrical shape or type of material, which can be done largely from memory, there is also the possibility of carrying out experiments in the mind—what physicists call 'thought experiments'. This type of process clearly calls for more than mere memory—understanding is also necessary. There are thus several ways in which an engineer might think about a structure and its behaviour, for instance:

- imagining geometrical aspects of a structure;
- direct knowledge of structural behaviour;
- thinking about possible structural behaviour;
- understanding structural behaviour;
- knowing how to use engineering knowledge.

In addition there is what can be called a 'direct knowledge' of engineering behaviour: the type of knowledge usually associated with craft skill, or the skill of *knowing how to* do something (e.g. swim, paint, make clocks, play a musical instrument; see

[Polanyi 1958, Hall 1978, Skemp 1979]). While the performance of such skill-based activities can involve conscious thought and self-awareness, it is not necessary. It is often only the philosopher who spends much time contemplating such skills. In *Zen and the Art of Motorcycle Maintenance*, Pirsig identifies:

> what's called *mechanic's feel*, which is very obvious to those who know what it is, but hard to describe to those who don't; and when you see someone who doesn't have it, you tend to suffer with the machine. The mechanic's feel comes from a deep kinesthetic feeling for the elasticity of materials. [Pirsig 1974:323]

As an example from motorcycle maintenance, Pirsig cites the ability to tighten up a nut with a spanner; the torque which must be applied depends upon the size of the nut and bolt, the pitch of the thread, the presence of lubricant, the size and type of spanner, the materials of which the various parts are made and the degree of tightness actually required—and yet a skilled mechanic does all this with great ease, confidence and accuracy. Every engineer will recognize this sort of a description of what is sometimes simply called 'know-how'. Part of this is a direct knowledge of materials:

> The foundation of engineering is knowledge of materials, not, as engineers are so often apt to preach, a knowledge of mathematics; . . . knowledge of what [materials] are made of, how they are made, how they are shaped, how you fit them together, how they stand up to stress, how they break, how they catch fire, how they react to all the various agencies of ruin which are perpetually nibbling at them, how in due course they fall down. [Harris 1961:130]

To this knowledge or 'feel' for materials there needs to be added also a 'feel for structure', since it is important not only which material is used but how it is distributed in an artefact—whether solid or a tube or flat or an I-shaped section; whether a string, unable to carry compression, or contiguous blocks, unable to carry tension; or whether a folded flat sheet or a shell curved in two directions. Such feelings for the world in which we live are learnt by all of us very early in life. Some people go on to develop them beyond childhood, learning by experiment, by following examples, and by being shown by experts (relatively speaking). Many eminent design engineers trace their appreciation of structural behaviour back to wooden blocks and construction toys (Meccano is often singled out especially).

As a complement to a feel for materials and structures there is, of course, our feel for loads, forces and stresses, as well as deflexions, stiffness and strength. It is difficult to describe what these feelings are, and we are confused by the fact that muscles give a misleading impression of force. A weight, for instance, appears to get heavier the longer we hold it; structures do not suffer this effect. Similarly there is the question as to precisely what it is we perceive as 'flowing' when we follow a load path from the point of application of a load, through a structure to the foundations, or through the muscles in our body. In terms of the concepts of mechanics, it is, in fact, not force or stress but momentum [Fuchs 1988]; however, this would seem counter-intuitive to many people. Our sensation of stiffness, likewise, relies on the kinaesthetic information about the position of our limbs which our muscles transmit to our brains. Since the muscles also 'measure' the force, a considerable amount of experience is needed to relate the changes in positional and force information (this is poorly achieved in people who are 'ham-fisted'). When it comes to full-size structures, much

of this experience is, of course, irrelevant since the magnitudes of forces are far greater than anything we can lift, and deflexions are usually far smaller than we can detect. An understanding of forces in structures can perhaps only be achieved by a combination of analogy with small models, and various qualitative intellectual concepts such as load-paths, bending, pre-stressing and stress fields.

Of particular value to the engineering professional is the careful observation of full-size structures under load. Herman Haupt, for example, mentions several examples of how his and his colleagues' understanding of structural behaviour was improved by observation of the many timber trusses he worked on as a railway engineer in the United States [Haupt 1853]. He mentions the different degree to which wood was crushed at different places in a structure; the way lead and zinc were squeezed out of compressive joints between cast iron pieces; the tightness or looseness of wedges used in the construction of timber bridges; the greater shear strains near the supports of a lattice truss than those at mid-span; the non-circular deflected shape of a bridge span under load, and so on—all this by men who, some of them, had very little knowledge of any relevant engineering science (theory).† Perhaps the most dramatic source of direct experience is the detailed study of structural failures. This can, of course, yield an accurate understanding of how a certain structure behaved in a particular set of circumstances and can highlight ways in which a stucture can fail which the designer had perhaps overlooked. However, failures do not necessarily tell much about how a structure was behaving before failure.

Whatever the source, the result of such learning is an ability to anticipate the manner in which an engineering structure might behave when it is loaded in various ways, which, although qualitative, can be extremely sophisticated—for instance:

- the manner in which the structure will deflect or tend to move;
- the ways in which it could fail and eventually collapse;
- the direction of forces acting on fixed supports;
- the approximate degree to which different parts will be differently stressed or will attract load;
- the relative importance of different types of structural action (e.g. arch action in truss bridges and beams);
- the interaction of different components of a structure;
- the manner in which different loads, applied simultaneously, can aggregate particularly severely.

This type of skill shares with other 'doing-skills' such as painting and playing music by ear, the characteristic of being very difficult to explain. Designers often describe how a structural idea or solution to a problem can appear as if from nowhere, like a tune in a composer's head, and only afterwards be adequately justified. When asked to explain how one knows that a certain structure will deform in a certain way, the answer might be something like: that is how structures behave; there is no more basic explanation. It is perhaps for this reason that phrases such as 'structural intuition' and

† For an entertaining insight into what it may be like be *be* a rivet, a 'five-sixteenths bulwark plate' a bilge-stringer or a deck beam in a ship's hull, see *The ship that found herself* by Rudyard Kipling [Kipling 1908].

'a feel for structures' are often used (see, for instance, [Ahm 1966; Mainstone 1973; McCleary 1980; Nervi 1956; Torroja 1967; Zuk 1960]).

Although the above discussion has been expressed in modern language, there is nothing which precludes a Greek or 12th-century person from having been similarly able to imagine structural behaviour. The extent of the knowledge would perhaps have been less in former times, but, within the context of contemporary engineering structures, not necessarily less effective. What is precluded is the possibility of, say, mediaeval designers writing anything down in terms we fully understand or which would verify to us that they understood how structures work in the way that we do. We should not, however, assume that people in past eras were not able to know about very sophisticated structural behaviour, although they might have lacked the concepts to communicate the knowledge and the quantitative tools to manipulate it.

As evidence of the high quality of structural understanding, one has only to look at the astonishing structures which have been built in the past; and if one looks outside the field of building structures, the finds are equally astonishing—an origami expert's knowledge of folded plate structures; a potter's expertise with thin shells; the ship builder's knowledge of most structural actions including three-dimensional tension structures (sails) and torsion (Fig. 7.1).

There has, not surprisingly, been some discussion of the value of a qualitative understanding of structural behaviour in the context of the education of engineers. Although the data is lacking to allow comparison with earlier times, some alarm has been sounded at the poor qualitative understanding amongst young structural

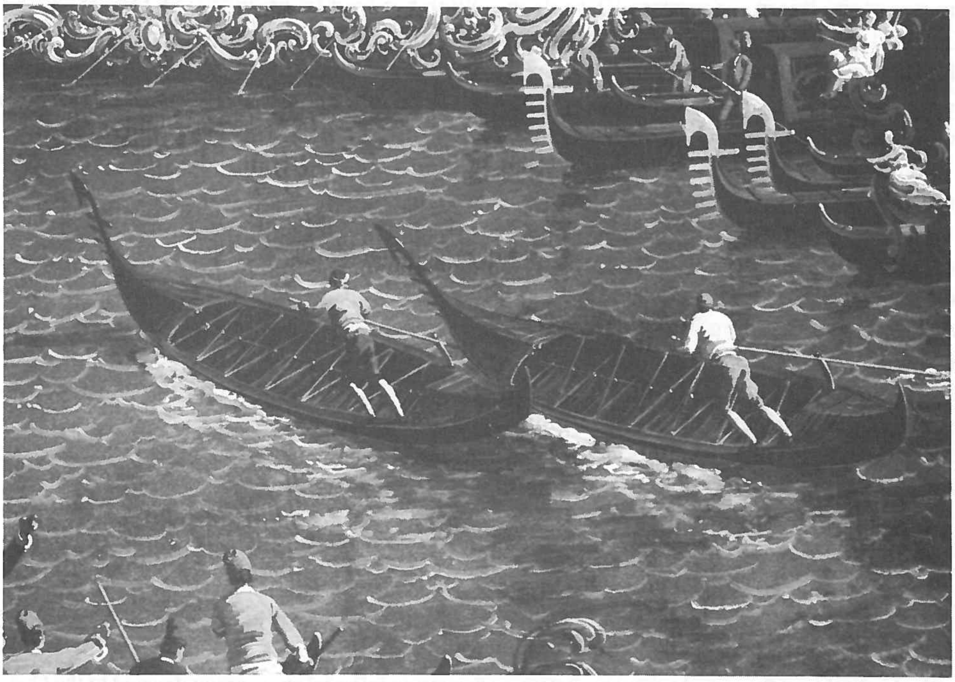


Fig. 7.1—Torsional stiffening of racing gondolas in the 1740s. [Canaletto] National Gallery, London

engineers [Brohn & Cowan 1977]. Although no-one has established the influence of changing patterns of play by young children (video games rather than Meccano) or of engineering education which concentrates too much on using computers to solve problems, or of a change in emphasis in education from design to analysis [Harris 1980; Pugsley 1980], many educationalists and engineers are concerned.†

7.2 CONCEPTUALIZING STRUCTURAL BEHAVIOUR

The 'direct knowledge' discussed in the previous section is not a self-conscious type of knowledge. Someone can perform a skill which displays an ability without being aware of how it is being achieved, and without being aware of any particular principles which are acting as a guide, and without being able to explain or teach the art to another, save by the practical tuition associated with teaching crafts and with the apprenticeship system. The first step along the road of self-awareness is the contemplation (*theoria* in Greek) of the practice of an art and is an intellectual process. It necessitates the use of concepts and the associated vocabulary to allow expression in a language. It is this ability to conceptualize that marks the transition from the direct knowledge to having the ability to think about, to understand and to communicate about structural behaviour.

The ability to conceptualize about engineering behaviour is not a recent phenomenon. The concepts of strength, stiffness and stability, *inter alia*, were familiar in Greek times, though not with our modern strictly quantitative implications [see Vitruvius c.25 BC; Alberti 1485; Duhem 1905; Mainstone 1959]. An impressive 6.15 metre stone beam, of inverted T-section, which tapered from deepest (0.95 m) at the centre to less at the supports, demonstrates that at least one ancient Greek engineer had an excellent comprehension of bending and how best to use material to achieve greatest strength [Coulton 1977:146] (Fig. 12.2). The Gothic cathedral designers demonstrated a similarly mature understanding of structures, especially their stability.

These fundamental engineering concepts remained substantially qualitative (but nonetheless effective) until the 'invention' of the concept of *force* (between 1200 and 1600) as what is now called a vector—a way of representing a load or thrust by means of a line of certain length, direction and position in space which allows the combined effect of two (or more) such forces to be evaluated mathematically. With the widespread understanding of the idea of a force there followed, relatively quickly, a host of other new concepts which led to earlier ones dying out or changing their use, for instance by becoming more specific and being defined in association with certain structural actions and the use of quantitative models.

The ability to use concepts of engineering successfully in discussing and explaining engineering behaviour gives a very direct indication of a person's understanding of, and ability to think about, engineering. However, to do this is not to engage in an objective and merely descriptive activity. The use of concepts and, more generally, language, in both thought and discussion specifies a standpoint from which a person is viewing something. To use a concept involves an act of interpretation. It involves

† The Institution of Structural Engineers in Britain has run an informal study group devoted to the Qualitative Analysis of Structural Behaviour since 1977.

not only seeing something (a sensation) but also 'seeing something as a . . . ' (an act of interpretation). This distinction is rather masked in many languages by there being two senses of the single word 'see', one a purely visual sense, the other incorporating also the sense of understanding.† 'Seeing something as a . . .' is a subjective activity and can thus vary from time to time, person to person, culture to culture and era to era.

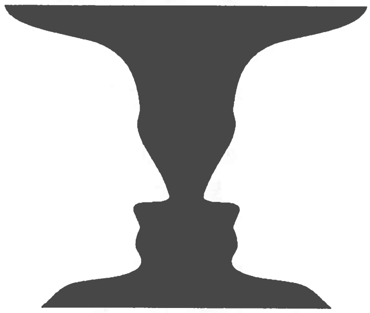


Fig. 7.2—An ambiguous figure

Consider, for instance, the sketch in Fig. 7.2 which can be seen by someone, now as a drinking vessel and now as two faces. Consider the ways in which a reader of Chinese and someone ignorant of the language will differently see and interpret a Chinese character. Consider also the different ways in which a cow is seen by a European and an Indian (as a potential meal and as a sacred animal, respectively).

Finally, consider the different ways in which a modern engineer and one from a few centuries ago (or a modern non-technical person) use the word arch. The latter uses it to define a certain type of shape, further specified, perhaps, as pointed, Gothic, Roman, Arabesque or elliptical. The former, however, uses it both to indicate a shape and a type of structural action involving compressive forces, a line of thrust, an inverted funicular shape and so on, and carrying with it an implied association with certain bodies of theory within statics and strength of materials. The modern engineer is, thus, incorporating an understanding of how the shape works as a structure and would therefore, at least initially, be more interested in whether it was a masonry arch or one made of a curved beam or truss, than in the precise shape of the arch, because of the different structural actions involved.

The changing use of engineering concepts can sometimes be traced through the way in which structures have been categorized in the many works which treat the history of the subject. In Cresy's *Encyclopaedia of Civil Engineering*, for instance, he categorizes 'most of the timber bridges built since the fall of the Roman Empire' according to twelve principles of design [Cresy 1865:1350–1353]. Of these twelve, we would nowadays categorize seven of them as arch bridges and three as truss bridges, and we would want to modify his 'suspension' category to exclude a timber truss stiffened and strengthened by means of a truss rod (in the old sense of the word). Colonel Long also used the term 'suspension' very differently from now—his Suspension

†For a full discussion of 'seeing', 'seeing as' and the 'duck-rabbit', a sketch which can appear now as a duck, now as a rabbit, see [Wittgenstein 1958:193-208; Hanson 1958].

Bridge was a simple cross-braced timber truss, half the diagonals of which were so constructed to be capable of carrying only tension forces [Long 1841:20] (Fig. 13.21).

An interesting illustration of the way in which concepts have changed can be observed when a copy is made, which in certain ways is deemed to be 'the same as' the original. Between the 5th and the 17th century a great many 'copies' were made, all over Europe, of the Holy Sepulchre at Jerusalem. What is surprising is that, despite documentary evidence that they were indeed intended to be copies, there are, to our eyes, astonishing differences both between the copies and the original and between the copies themselves. It is far from clear to us in what ways they were meant to be copies—an octagon has replaced a circle, eight columns in a circle have replaced twelve, semi-circles, rectangles and squares seem to be interchangeable, and both plan and elevation appear radically different. On the other hand, the mediaeval sources do stress the religious and liturgical significance and the iconography of the buildings and the value of them truly being copies of the original:

> The only justifiable conclusion seems to be that the mediaeval conception of what made one edifice comparable to another was different to our own. Mediaeval man must have had *tertia comparationis* utterly at variance with those to which we are accustomed. [Krautheimer 1942:3]

Observations of this kind should counsel against superficial readings of old texts and, more generally, should warn of the ever-present tendency to carry our own concepts with us when we view other times and places of history. We will meet this point again in later chapters.

In general terms, the main consequence of the notion of 'seeing as' is that the whole attitude of someone contemplating an actual or a projected engineering structure can be utterly different from the attitude of another, apparently doing the same. It is as if each person were viewing a coloured scene while wearing differently tinted spectacles. The problem is that we are usually not aware of the particular tint of our own conceptual spectacles, and it is very difficult, if not impossible, to remove them to get an untinted, objective look at the scene. An example of the above tendency is found in a common question concerning the nature of the design rules in times before the use of modern theory—were these rules in any way structural or were they always 'merely' architectural? This question is, however, a loaded question: it implies that the concepts 'structural' and 'architectural' were known and used in Greek times or in the 12th century in the manner they are used today. This was not the case: the question is therefore meaningless and cannot be answered except by saying simply that they were rules used to design buildings.

7.3 THE MODELLING OF STRUCTURES

The process of 'seeing as' is especially important in design procedures and, more generally, in the process of thinking about and understanding the behaviour of engineering structures. The very process of representing a real structure, a real load or a real material by a model (in the modern sense of the word) consists in creating what is believed to be a valid or useful representation. At different times in history the models have involved, in varying degrees and combinations, small-scale and life-size

physical models of structures as well as various mathematical models—geometric, numeric, algebraic, trigonometric and statical. The state of the art in engineering design, at a particular time, is indicated by the types of models which were seen as able provide an adequate description and justification as the outputs of the design procedure. In general, the appropriateness or adequacy of a chosen type of model depends, either implicitly or explicitly, upon the various theories which have sought to explain the way nature works during the last two thousand years or so—the use of a model is an expression of the logical, mathematical and metaphysical formalization of a theory.

The reader should be reminded here that long before the use of formal logic and precise quantitative mathematics during the last three hundred years or so, informal (intuitive) logic and approximate relationships between quantities were universally used—any Greek engineer could have told you 2000 years ago that a rectangular beam deflects much less when placed on edge and that doubling the length of a beam considerably increases its deflexion under load.† While not precise, such knowledge is extremely useful. In fact, it forms the basis of qualitative structural understanding even today—while we may nowadays know that the stiffness of a rectangular beam varies with the cube of its depth, we are no more able to carry this precise mathematical relationship in our mind than the Greek engineer could. We can, however use it when a precise description or justification is required.

The model of any structural problem which a designer may be tackling, comprises three independent models:

- a model of the structure itself
- a model of the material of which the structure is made
- a model of the loads to which the structure may be subjected

Each of these three models may be of three types:

- a physical model (small-scale or full-size)
- a qualitative model
- a mathematical model (geometric, statical etc.)

Each of these nine types of model will represent actuality in different ways—more or less accurately, more or less usefully and so on. Each model will have some properties which are good representations of reality (positive), some which are poor (negative) and some which are irrelevant (neutral). For example, a laboratory model of a pin-jointed truss bridge may give a very good representation of the overall geometry, quite a good representation of the joints and their freedom to rotate, but a very poor representation of the dynamic behaviour of a real bridge. The colour or surface hardness of the model may be poor representations, but this is likely to be totally irrelevant.† To create a certain model of actuality is to perceive actuality in a particu-

† It is interesting that recent developments in the field of computer-based knowledge systems (expert systems) and in the field of structural safety [Blockley 1980] make considerable use of 'fuzzy logic' to try to regain the useful imprecision which human beings are so easily able to handle in their informal languages and logic.

† For a full discussion of these issues as applied to the use of models in science, see [Hesse 1963].

lar way: it also involves making appropriate assumptions, simplifications and approximations in order to simplify actuality sufficiently to allow the creation of the model. This means that there are a great many steps in the process of creating a useful model of structure, materials and loads, and hence a large number of stages at which discrepancies may arise between the real world and the properties of the model (Fig. 7.3).

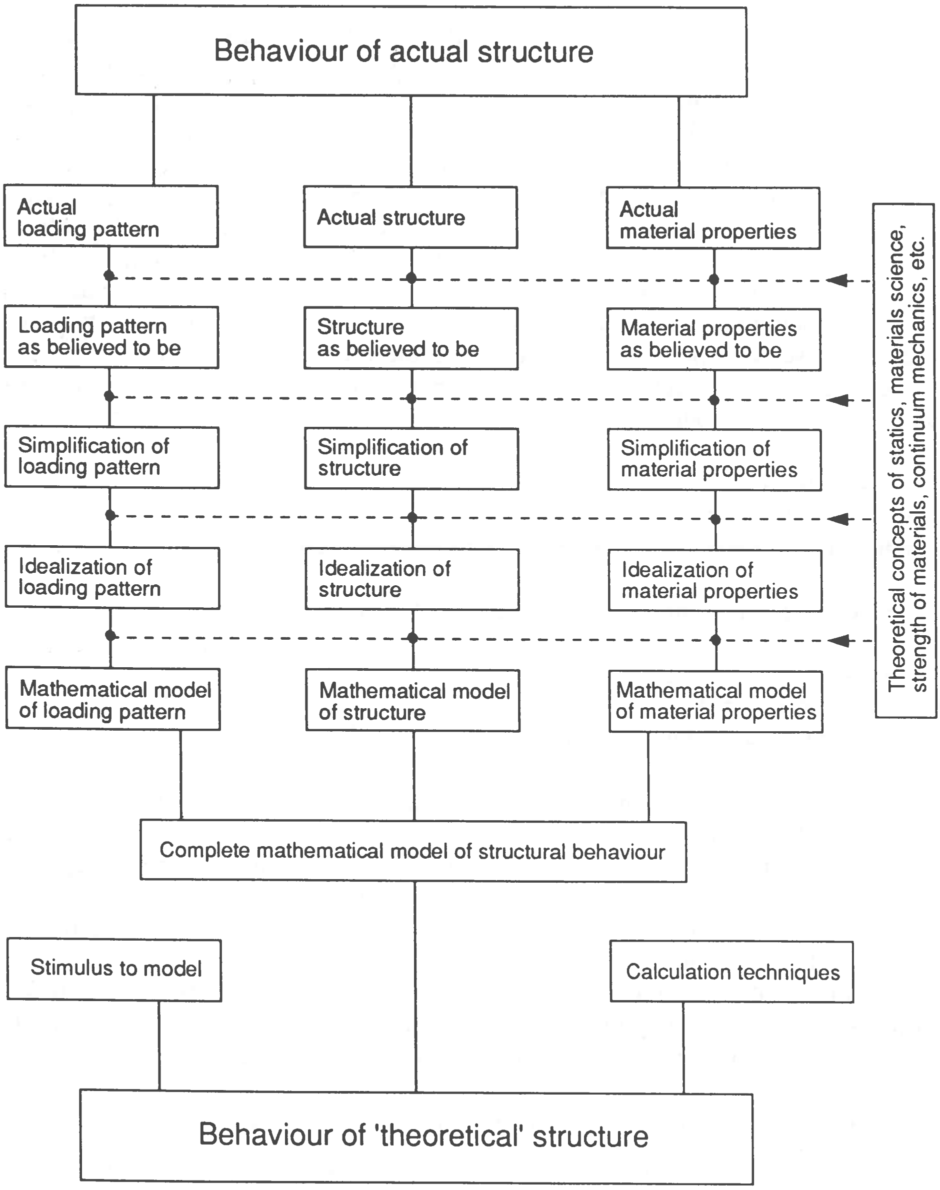


Fig. 7.3—Stages in modelling a structure

Sometimes the qualitative model chosen to represent a structure can be known to be wrong or grossly inaccurate, but nevertheless felt to be useful to the designer in helping to understand how the structure behaves and to develop a useful mathematical model. A shell arch dam can, for instance, be modelled as a series of horizontal arch-shaped strips, or as a series of vertical strips pinned at their lower end. Yet these are simply two ways of thinking of how the material of the shell is behaving: the structure is not behaving in two different ways.

On the other hand, some models make approximations the consequences of which cannot easily be understood. In computer programs that use a finite element method of analysis, the conditions of equilibrium for each element can be set down and the boundary conditions of ajacent elements then matched by an iterative process of refinement of patterns of stress and strain distribution. However, the way in which the elements are chosen (both their size and shape) can lead to different results. Different assumptions made at this stage, therefore, can have considerable implications about the way in which a structure is portrayed as working. Thus, two models could both be believed to be correct and yet not agree with each other. Unlike engineering scientists, designers have the option, often to a very great extent, to adjust actuality to suit the model, as well as choosing a model to suit actuality. As we saw in §5.1.2 when discussing the justification of proposed designs, this is an important way in which mathematics and scientific theories can influence the design of structures. We shall see in the chapters which follow that choosing between different possible models of actuality is of crucial importance to the work of both engineering scientists and designers and to the historical development of science and design.

7.3.1 Ways of seeing trusses

The importance of how a structure can be 'seen as' working in a certain way, and hence what sort of a model may be appropriate, can be illustrated by a brief survey of some of the developments relating to the design of truss bridges and roofs over the last two centuries or so and the various types of model chosen to represent this type of structure. (The story will be taken up in a different context in §13.3. Here the story will be restricted to models involving mainly statics.)

The idea of taking moments and the method of finding the resultant of several forces using the triangle of forces, were already developed by the year 1600. By the late 18th century, the triangle of forces was appearing not only in mechanics textbooks but also in some books of carpentry and roof construction. Modern beam theory was well developed by 1800 and basic statics and strength of materials were used in the investigation of suspension bridges in the the early 1800s. Yet the first examples of investigating the forces in trusses by considering the equilibrium of the joints (now learnt by engineering students in their first term at university) was only first undertaken in the 1840s—some 250 years after it was theoretically possible. Why did this take so long?

This type of story, which now seems almost incomprehensible, is, in fact, not uncommon. Similar 'delays' are found in other branches of mechanics—it was nearly a century after the appearance of Newton's *Principia* that the second law of motion was demonstrated by means of an experiment using Atwood's machine (a trolley accelerated by a constant force) [Kuhn 1977:189]. There was more than a century gap between Galileo's first treatment of the motion of a pendulum with a point-mass for

a bob and the use of his model by Huyghens to treat the motion of an extended-body pendulum [Kuhn 1970a:190; Truesdell 1967; Reynolds 1973] (Daniel Bernoulli then quickly proceeded to use this extended-body model in the context of the flow of water—a much more 'complex' problem solved by analogy).

The first successful use of some new mathematics to relate to a physical situation involves a great leap of imagination. The very act of interpreting the mathematical concepts as having a physical significance in the real world is extremely difficult — certainly much too difficult to be achieved by most of the 'practical men' who were making bridges and roof trusses.

The problem with real bridges and roofs is that they appeared to the people making them as utterly different from the highly idealized rods of infinite thinness joined at frictionless pin joints which the mathematicians worked with. To construct a mathematical model of a real timber truss, accurate in every way, is, even today, a practical impossibility (ignoring for the moment the philosophical problem of how one would know it was 'totally accurate'). The dramatic breakthrough in using engineering science in the design of trusses came with the realization that one could make what at first sight appear to be enormous, even unreasonable, simplifications and approximations. This approach to modelling was first generalized as late as the 1850s by Rankine (see §4.2).

It might seem rather curious to the modern reader, perhaps, that the first attempts to make use of engineering science in the design of truss bridges was to treat them as beams, modifying the well-known relationship between the dimensions of a beam and its strength by an empirical constant which took account of the 'holes' in it. This is not so odd, however, if one considers that beam theory was already well-developed by the end of the 18th century. Navier published his famous textbook of engineering science, in 1826, based on his lectures at the École des ponts et chaussées in Paris and in the section devoted to timber truss bridges—parallel top and bottom chords with St Andrew's cross bracing—he calculates both their stiffness and strength as if they were I-beams with no web and goes on to comment:

> L'hypothèse sur laquelle ces formules sont fondées ne peut être realisée, lorsque les pièces sont placées parallelement, qu'autant que ces pieces sont assujetties l'une à l'autre par un système de traverses et de croix.† [Navier 1833:328]

This approach continued for the next 20 years or so, as far as anyone used these ideas at all to help them design actual bridges—we have little evidence that they did. In 1845 a calculation of the strength of some American railway truss bridges used similar ideas [Ghega 1845] and even the fully statically determinate Warren truss was first analysed in the same way [Doyne & Blood 1851]. So we see that during this period, engineers looking at truss bridges (most of which were highly redundant) quite literally saw them working as beams, with tension, compression and shear forces following the pattern of the internal stresses in a beam.

At some time during the mid- to late 1840s there arose an utterly new way of considering these same types of bridges. It consisted in seeing the bridges as made up of many separate, idealized pin-jointed members acting in tension or compression. The

† The hypothesis on which these formulae are based cannot be realized—while the chords are indeed placed parallel, they are fastened one to the other by a system of posts and crosses.

structures, most of which had cross-bracing and were therefore statically indeterminate, were simplified by ignoring enough members (depending on their construction) to make them statically determinate; they could then analysed by considering the conditions of statical equilibrium of the whole or any of their parts [Whipple 1847; Schwedler 1851; Culmann 1851].

This technique led in turn to a more general means of dealing with other redundant structures such as lattice trusses and those which combined two structural elements, such as a truss stiffened by an arch. The structure was broken down into two independently acting, statically determinate systems each carrying half of the loads [e.g. Haupt 1853]. Only in the late 1860s did the material properties enter into the analysis in the manner in which statically indeterminate frameworks are treated nowadays.

The French engineer Clapéyron even used this same technique of breaking structures down into easily imaginable statically determinate sections to illustrate the way in which an ordinary statically determinate truss can be thought to be working (see Fig. 7.4).

By 1880 it had come to be felt that the pin-jointed model of a truss was too crude or approximate to allow further refinement - it was well known that most iron structures now had rigid rivetted joints and timber frames had always employed stiff mortised joints. A variety of ways were developed to enable the trusses to be seen as pin-jointed during part of the process of analysis and as rigid elastic structures at other times—the whole notion of secondary stresses was born [Timoshenko 1953:322]. Nowadays, it would be common to see truss bridges working as a series of elements being elastically deformed and storing energy in the process—the concepts of virtual work and energy allow a much more generalized approach to understanding structural behaviour than the previous approaches of analogy with beams and the idea of the pin-jointed model (see also §13.3 on truss design.)

Finally, brief mention should be made of an unusual modern approach to the overall safety of certain timber structures against collapse which considers the

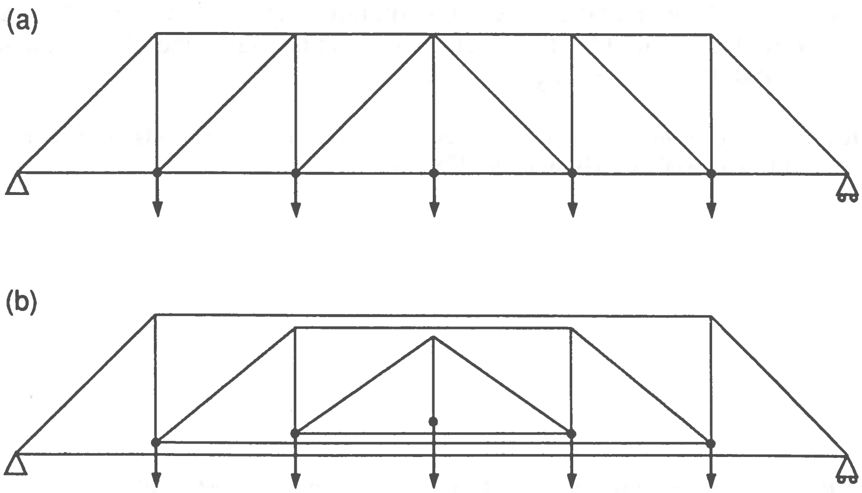


Fig.7.4—(a) A statically determinate truss. (b) Clapéyron's view of the same truss [Molinos & Pronnier 1857:66]

behaviour of the structure as a whole, and totally avoids the need to establish the exact way in which the structure is working. Indeed, the structure is seen as not having a precisely definable way of working at all. This radically different way of looking at a structure's behaviour has been applied to the 550-year-old hammerbeam roof truss at Westminster Hall [Heyman 1967]. The author argues that in such a redundant structure there is no meaning to the idea of an 'actual state of forces and stresses' since, even though they could conceivably be established at a certain time, minute movements of the supports totally invalidate the assumptions made about the way the structure might be carrying the loads. Heyman goes on to use a 'limit state' analysis which was originally developed for use with steel frame structures. This establishes a 'lower bound' or 'safe' solution which will therefore be the minimum strength of the roof.†

In this extract from the history of trusses it is tempting to see the development simply as one of gradual 'improvement'. While there were improvements, this is not the principal point. Rather, the various different ways of seeing trusses are alternatives, all still familiar to us, of conceiving of the way a structure behaves. This point is especially relevant today when there is such a wide range of different models of structures to choose from and different ways of analysing structures, and unlike any other period in our history, they are often hidden from view inside a computer. As computers become more widely used, so more and more people will be using them who are not aware of the way in which they model materials and structures or the assumptions and approximations which are inherent in all techniques of modelling (all this is in addition to the dangers of errors in the programs themselves). The obvious answer is, of course, to cross-check results obtained in two different ways (one by hand, perhaps), but it is all too easy to become a bit lazy and no amount of checking will assist in the process of selecting an appropriate model in the first place.

The importance of the way in which an engineer thinks about structures and the choice between different models and ways of looking at structures can hardly be overstressed: it controls, quite literally, *what* they see and it is by means of this creative act of seeing the problem, that the route to the solution can become apparent (see [Cross 1935a, 1935b; Ferguson 1977]. It is surely in this act that the engineer's deepest understanding and skill lies. As Hardy Cross said:

> Design involves sound judgement as well as stress analysis; and judgement is more important. (Cross cited in [Billington 1973:164])

† For further discussion of this approach see Chapter 11. Incidently, the use of this radical approach caused considerable disagreement in the discussion following its presentation—indeed no less disagreement than had occurred at a similar discussion about the same structure, more than a hundred years earlier [Morris 1844, 1850].

8

Design seen as a skill

We are now in a position to return to an issue touched upon earlier and to look at what type of a process design is. In §1.5 it was suggested that structural and civil engineering should be considered as arts; that is, they are not science—the pursuit of knowledge and understanding, nor even applied science, whatever that may be: as Professor Alan Harris has noted, engineering is no more applied science than painting is applied chemistry, despite the fact that a knowledge of how to mix paints is highly desirable [Harris 1975:17]. But what of the intellectual side of engineering, the process of design. It can hardly be said to be a practical art like painting or pottery.

In Chapter 5 we looked in some detail at the activity of design with the aid of two types of models of the design process—one project-related, the other centred on the design procedure. In Chapters 6 and 7 we looked at the knowledge which might be relevant to engineering design and the way that knowledge relates to how we think about structures. If we stop at this point, however, we would be taking account only of factual knowledge, that which it is possible to *know*: but there is also *knowing how to* do something, and this can include intellectual as well as practical activities.

The key to understanding the nature of design can be found in focusing upon the activity itself and the fact that it is performed by people carrying out a number of skills. Rather than describing design in terms of anonymous data, stages of a project and results, the activity is described in terms of thought processes and human actions which the designer knows how to do, such as:

- study and analyse (the brief and other data)
- conceive and propose (ideas and possible solutions)
- evaluate (several possible solutions)
- choose (an appropriate solution)
- analyse (likely structural behaviour)
- justify (a chosen design)
- communicate (the design to others)

and so on.

To consider design as a human skill places the person at the centre of attention in the same way as when we call structural engineering, as a whole, an art. By seeing design as a skill we can also quickly start to see how we might assess the quality with which it is carried out, both in terms of the process and the final result; and we shall also start to see implications for the ways in which people might learn or be taught the skill, or rather, skills.

Most of the raw materials with which the structural engineer works have already been introduced—empirical data, branches of mathematics, design procedures, qualitative knowledge of materials and structural behaviour, theories of engineering science, and so on; so too have the products—a description and justification of a proposed design. The skills of the structural engineer relate to the handling of these various bodies of knowledge in order to achieve the desired ultimate end—a building fitting the client's brief.

The skill of being a structural engineer is something entirely internal to a particular individual and hence not able to be looked at objectively and compared directly with the skill of another. As with many other skills, it is usually not possible for the possessor of a skill to describe it fully, to understand it, or to explain precisely which rules, if any, are being followed. It is therefore not easy to pass it on to someone else—a skill can only be learnt by personal experience. For this reason, incidentally, the ultimate value of many so-called computer-based 'expert systems' must be doubted or, at least, carefully questioned. The skill of the structural engineer is, at a most fundamental level, knowing how to select and make use of appropriate data, mathematics or tools by way of proceeding with conscious intention towards a known goal. Such 'behaviour' is, by definition, not capable of automation.

All this is in great contrast with much mathematical and scientific knowledge, which by its very nature is objective, public and easy to communicate to others.† Perhaps the most significant differences between scientists and engineers (apart from their different goals) are to be found not in the differences between their knowledge, which they often share, but in the skills which they each have, which they seldom share.

At all times in the history of structural engineering, there have been three main areas of skill which different designers possess to different degrees:

- the ability to come up with innovative and highly appropriate structural solutions to both familiar and new problems;
- the ability to take a design from a basic or general concept to the level of fine detail design;
- the ability to create designs which can be built easily and cheaply.

Within these broad areas, a number of more specific skills which a structural engineer might possess can be identified. The list given below is intended to be representative rather than exhaustive and, since they are all likely to be drawn upon at any stage of a project, no attempt is made to present them in categories or sequentially. To avoid a totally random list, however, they are presented, very approximately, according to

† For excellent and rare discussions of skill and its relationship to knowledge, see [Polanyi 1958:Ch.4] and [Skemp 1979:Ch.11].

their generality: no heirarchy of importance is implied. Although many of the skills have been expressed using modern concepts, *all* of them could be applied to people engaged in building design at any time in the past—one or ten or twenty centuries ago (remember, 'mathematical model' includes geometrical model).

- to have intention and assess progress towards a predetermined goal: to be able to detect which way is forward;
- to analyse a problem logically;
- to choose and mix appropriate skills in order to progress towards the desired goal;
- to learn in different ways, both from one's own experience and from the experience of others;
- to communicate ideas and information to others using appropriate technical language and concepts;
- to develop peer group identity, for instance for the purposes of developing common standards and work methods, and to facilitate learning by the group as a whole;
- to recognize and evaluate, at any stage of a project, issues of relevance to the structure of the building;
- to select, from an almost infinite amount of possible data about materials, products, processes and costs, that data which is appropriate at a particular time;
- to understand the implications of the concerns of other professionals (such as cost, services, buildability, durability, fire resistance etc.) upon the building structure, and vice versa;
- to have a feel for materials, their properties and behaviour;
- to have a feel for structural actions and behaviour;
- to imagine the likely loading on a structure and the corresponding behaviour of a structure under load, both as a whole and at the level of individual elements;
- to imagine an unbuilt structure both geometrically and in terms of chosen structural actions;
- to imagine possible collapse mechanisms for individual elements, due to particular loading conditions, and their consequences leading up to total structural collapse;
- to imagine, and hence be able to prevent, other different ways in which a building structure can fail (in the widest sense of the word);
- to conceive innovative and highly appropriate structural solutions to both familiar and new problems, either drawing from precedent and experience or *ab initio*;
- to conceive a structural solution at a level of detail appropriate to different stages of a project;
- to imagine as many as possible of the consequences of a structural decision or choice between alternative designs;
- to think both qualitatively and quantitatively about loads, materials and structures and to switch easily, appropriately and at will between both modes of thought;
- to create one or more mathematical models of an imagined or actual structure, using geometry, algebra, trigonometry, statics and so on;

- to make appropriate assumptions, approximations, idealizations and simplifications of actuality in order to be able to build both mathematical and physical models for use in a design procedure;
- from a variety of possible mathematical models of loads, materials and structure, to evaluate and select ones appropriate to a given case;
- to know and use a range of standard design procedures;
- to recognize when a proposed structure can or cannot be designed using standard or existing design procedures;
- to select or modify an appropriate existing design procedure, or create a new one for established types of structure;
- to develop an entirely new design procedure for a type of structure which has never before been designed of built;
- to evaluate the relative power of different methods of justifying a proposed design using precedent or physical or mathematical models;
- to know when a physical model will help to develop a design or to understand a structure's behaviour;
- to be able to build scale models to test structural ideas and behaviour;
- to interpret the results obtained from a scale model for use at full size;
- to choose appropriate performance criteria against which a structure can be assessed, and limits which it must not transgress;
- to evaluate the performance of a proposed structure and hence compare the performance of alternative proposals;
- to ensure that a proposed solution satisfies non-structural criteria, such as thermal and acoustic behaviour, buildability and durability;
- to communicate and know when to communicate with specialists from other disciplines such as architect, contractor, services engineer;
- the ability to take a design from an outline conception to the level of fine detail design;
- to recognize, from experience, ways of simplifying the problems of design and seeing short cuts to approximate answers: for instance by identifying highly stressed, sensitive or problematic parts of a building structure which will require particular attention, allowing time and effort to be saved by treating the rest of the structure as a relatively standard solution;
- to select and use a variety of tools, such as computer programs, codes of practice and physical models, to assist in the design procedure, for instance by making it quicker, less arduous or more thorough;
- to carry out and record calculations justifying a design, for instance, for independent checking;
- to use a variety of calculating devices and, nowadays, types of computer software;
- to imagine how an artefact could be manufactured, assembled and, where appropriate, maintained;
- to assess the buildability of a proposed design; to create designs which can be built easily and cheaply;
- to draft appropriate and sufficient drawings to represent and communicate a building design to those persons who need to know it;
- to draw up effective written specifications for materials, workmanship and

matters relating to method of construction and temporary works;
- to know when the process of design has been completed in a satisfactory manner.

Seeing design as a skill helps to resolve some of the issues raised in Chapter 3 concerning the type of education which might be appropriate to engineering and establishes a basis upon which the quality of design, both the process and the product, can be evaluated and criticized. These matters will be taken up in later chapters after we have looked at the nature of historical development in engineering science and design.

9

Engineering science and its historical development

In the first three chapters of the book a number of problems concerning the nature, history and pedagogy of civil and structural engineering were identified. As a first step towards solving or avoiding the problems, it was argued that their origin lay partly in the traditional theory/practice classification of engineering. As an alternative, the engineering science/design/construction classification was proposed. The principal problems concerning the history of engineering science, mentioned in Chapter 2, were that the conventional approaches to history lead to the following implications:

- science appears to be purely cumulative and to develop along a series of lines which merge, like the tributaries of a river, all leading towards the present day state of science;
- the development of science is totally logical;
- there were no theories of any interest or significance which tried to explain nature before the advent of modern science in the middle of the 18th century;
- all theories and concepts which have now been discarded were, even at the time they were proposed, 'wrong'; they are thus regarded as inferior to the theories which replaced them, and any importance they had during the time of their currency tends to be overlooked.

These problems are very similar to those encountered by Thomas Kuhn in his study of the history of science—especially physics and chemistry. This chapter presents a summary of the background to, and development of, Kuhn's ideas, as presented in his book *The Structure of Scientific Revolutions* [Kuhn 1970a]. This will enable a little of the history of engineering science to be discussed in a Kuhnian manner, and will provide a foundation for adapting Kuhn's ideas to the history of engineering design.

9.1 BACKGROUND TO KUHN'S MODEL OF HISTORICAL DEVELOPMENT

Kuhn developed his ideas as a direct result of encountering certain difficulties in understanding the history of science as he saw it, in terms of the historiographic methods normally used by historians of science. In the introduction to his book, Kuhn discusses the inter-relation between history and the image we now have of science. It is worth reproducing part of this introduction:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each scientific generation learns to practise its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself.
>
> Even from history, however, that new concept will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by the unhistorical stereotype drawn from science texts. Those texts have, for example, often seemed to imply that the content of science is uniquely exemplified by the observations, laws, and theories described in their pages. Almost as regularly, the same books have been read as saying that scientific methods are simply the ones illustrated by the manipulative techniques used in gathering textbook data, together with the logical operations employed when relating those data to the textbook's theoretical generalizations. The result has been a concept of science with profound implications about its nature and development.
>
> If science is the constellation of facts, theories, and methods collected in current texts, then scientists are men who, successfully or not, have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. Concerned with scientific development, the historian appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text. Much research has been directed to these ends, and some still is.
>
> In recent years, however, a few historians of science have been finding it more and more difficult to fulfil the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen first discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. Perhaps science does not develop by the accumulation of individual discoveries and inventions. Simultaneously, these same historians confront growing difficulties in distinguishing the 'scientific' component of past observation and belief from what their predecessors had readily labelled 'error' and 'superstition'. The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs

> are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion. The same historical research that displays the difficulties in isolating individual inventions and discoveries gives ground for profound doubts about the cumulative process through which all these individual contributions to science were thought to have been compounded.
>
> The result of all these doubts and difficulties is a historiographic revolution in the study of science, though one that is still in its early stages. Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e. his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint—usually very different from that of modern science—that gives those opinions the maximum internal coherence and the closest possible fit to nature. Seen through the works that result . . . science does not seem altogether the same enterprise as the one discussed by writers in the older historiographic tradition. By implication, at least, these historical studies suggest the possibility of a new image of science. This [Kuhn's] essay aims to delineate that image by making explicit some of the new historiography's implications. [Kuhn 1970a:1–3]

The reader will perhaps have noticed a parallel between the circumstances surrounding the birth of Kuhn's ideas and the 'problems' concerning the current 'image' of civil and structural engineering and the historical treatment of these subjects. In the rest of this chapter I shall summarize the nature of the 'historiographic revolution' and of the 'new image of science' to which Kuhn's introductory observations finally led.

9.2 'NORMAL SCIENCE' AND 'SCIENTIFIC REVOLUTIONS'

As an alternative approach to seeing scientific development as a 'process of accretion' or 'development-by-accumulation' Kuhn proposed a model of evolution based upon two distinct types of change. First, there are, indeed, periods of development which are largely cumulative in nature. These he calls periods of 'normal science'. There are also, however, other periods which separate periods of normal science and are discontinuities in the evolutionary process. They consist of changes which lead to radically new ways of looking at the world. These discontinuities he calls 'scientific revolutions'.

An excellent example of a scientific revolution is that due to Copernicus—the change from seeing the universe as centred at the earth to seeing it as heliocentric. The main point about this revolution was that it involved no new experimental evidence and, in fact, initially led to less accurate predictions of the positions of the planets than the earlier theory of Ptolemy could give. The revolution was to interpret the existing evidence in a new way, incompatible with the old way. In modern language, he created a new mathematical model of the universe.

Kuhn defines his idea of normal science in terms of two other ideas. A branch of normal science is the activity of a particular 'scientific community'. A scientific community is, in turn, seen as a group demarcated by their common beliefs, aims, theories, textbooks, vocabulary, experimental apparatus and procedures, and the very research problems themselves. This totality of what the members of a scientific community share, Kuhn calls their 'paradigm' beliefs, aims, theories, etc., or, as he frequently abbreviates them, their 'paradigms'.

9.3 THE REVOLUTIONARY CYCLE

The revolutionary cycle of scientific development can be traced by starting in a period of normal science. The world is viewed and interpreted in terms of the currently held paradigm beliefs and the whole activity of the scientific community is governed by these paradigms. This activity comprises what have been identified as crucial or paradigm experiments with the aim, for instance, of obtaining more and more accurate values of certain important data (e.g. physical constants) and of applying the paradigm concepts and theories to more and more cases, in more and more sets of circumstances—in other words, making the theories more powerful and generalizing them. Kuhn calls this process 'paradigm articulation'.

The characteristic of the various activities of normal science is that they are undertaken in the belief that the problems addressed can be solved using the tools available within the current paradigms. Kuhn thus likens normal science to puzzle solving, a simile which, incidentally, goes a long way towards explaining the attraction which both theoretical and experimental science hold for their aficionados—a puzzle must be assumed to have a solution and the means of solution must follow a particular set of known rules. Also, the existence of a known solution to a puzzle does not preclude the search for a more elegant one and 'it is no criterion of goodness of a puzzle that its outcome be intrinsically interesting or important' [Kuhn 1970a:36]. This same sentiment has been expressed by at least two persons who have contributed much to engineering science:

> The history of the mathematical theory of Elasticity shows clearly that the development of the theory has not been guided exclusively by considerations of its utility for technical Mechanics. [Love 1906:30]

> 'It can be said with little fear of contradiction that the engineering scientist will always be willing and eager to continue his work whether it is used or not by his practising colleagues. [Pippard 1956:162]

With the acceptance, adoption and development of its set of paradigm beliefs, theories and so on, a scientific community also develops a set of criteria for identifying and choosing problems which can be assumed to have solutions. Indeed:

> one of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving. [Kuhn 1970a:37]

The activities of normal science represent, then, a period of relative stability and calm. Development during such a period is, by and large, cumulative. It involves a

natural progression from one problem to another. The results of investigation can be more or less anticipated. Normal research does not seek, nor expect to find, major novelties of fact or theory; nor does it seek to undermine or challenge the foundations of the accepted paradigms.

Within the usual calm and predictable work of normal science there is, however, the possibility that new phenomena may be encountered, either directly out of research as unanticipated results or, more or less, by chance. These new phenomena may not immediately be able to be explained in terms of the accepted paradigm beliefs and theories. Such occurrences will appear as 'anomalies'; nature will seem to have violated the paradigm-induced expectations which govern normal science.

The discovery of an anomaly may result in one of several consequences. It may be attributed to 'experimental error' or to an inadequate experimental technique. Or the anomaly may be accounted for, *ex post facto*, by a previously untried type of paradigm articulation, or perhaps by means of minor adaptations or modifications to the current paradigms. Both of these outcomes would eventually be claimed as confirmation of the power of the paradigms which form the basis of the scientific community's way of life.

On the other hand, an anomaly may continue to defy attempts at explanation and may be set aside as a currently insoluble problem, or simply ignored. The acceptability of this course of action will depend upon the perceived importance of the anomaly. To someone who perceives it as being crucial, the particular branch of normal science may be judged to be in a state of what Kuhn calls 'crisis'. As more anomalies arise, and as more persons come to believe the anomalies to be crucial to the survival of the set of accepted paradigms, so the calm of normal science becomes a state of panic and chaos. More and more attempts at accounting for the anomalies fail, or are seen as less and less plausible, until eventually someone is moved to seek a radically different way of looking at the problems, in other words, to challenge the very paradigms held by the scientific community. Such a fundamental upheaval Kuhn calls a 'scientific revolution'. He draws the analogy with political revolutions in which a significant number of persons living under a political system decide that change is required which is not possible within that system:

> Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favour of another. [Kuhn 1970a:93]

Using another analogy, if normal science is playing a game according to certain rules, then a scientific revolution is changing the rules of that game. A revolution results in a radically new way of looking at, and of conceptualizing the world. It involves breaking through a barrier, whose very existence was embodied in the previous paradigm beliefs. A scientific revolution is thus, first and foremost, an intellectual phenomenon.

The so-called 'discovery' of oxygen illustrates this previous point. It is usually supposed that someone discovered oxygen in the same sort of way that someone (else) discovered the source of the Amazon—it was there waiting to be found. Yet, the most detailed historical investigations fail to establish which person it was, and in what year he did so [Kuhn 1970a:53ff] (unlike the case of the source of the Amazon).

Scientists of the 1770s were united in their aim to establish the nature and properties of 'phlogiston', the substance which was believed to be present in all materials and which was released during combustion. Their investigations resulted in more and more inexplicable data—inexplicable, that is, in terms of a substance of the type phlogiston was supposed to be. It was only over a period of several years, and through the work of several experimenters, that the idea was formed of a different sort of substance which might lie behind the phenomena associated with combustion—an isolable gas which was a constituent of air. As with the Copernican revolution, it was not initially new experimental evidence which led to the revolution. It was, rather, that the state of crisis forced the need to look at old evidence in new ways.

With the proposal of a radically new way of looking at the world, and the creation of a new set of hypotheses and theories, so, eventually, a new scientific community is established with its new set of paradigms—new theories, new concepts and new problems. Of particular interest, however, is the process by which such a new community grows. How is it that a member of the old community, believing in the old paradigms, could ever be persuaded to give them up in favour of the newer ones? And here we are not talking about giving up trivial matters—some people who dared suggest the earth orbited around the sun were even put in prison! Perhaps fortunately for engineering scientists, their subject has never aroused such emotions, but we should perhaps be grateful that they sometimes were aroused—while Galileo was in prison for supporting his Copernican views with the suggestion that Jupiter had four of its own moons, an idea which conflicted with the image of the perfect known universe, one sun, six planets and one moon, he wrote his *Two new sciences*, the first book on the science of strength of materials.

9.4 THE CHOICE BETWEEN THEORIES

According to the conventional view of scientific development as a wholly logical affair, people would be persuaded to adopt the new ideas by means of cold logical argument. This view suggests that new theories are, somehow, logical consequences of previous scientific work. The mechanism which Popper suggests is that scientific theories are put to the test by specific experiments designed to try to falsify them and that, when there exist rival theories to explain certain observed phenomena, experiments can be devised to reject all except one of them [Popper 1959].

Kuhn, by contrast, portrays new theories as radically new ways of looking at, interpreting and explaining the world. In particular, he stresses the fact that the new paradigms born of a revolution are both non-deducible and counter-intuitive (i.e. counter-inductive).† This fact is most important because of the implications it contains for the choice between rival paradigm theories. With incompatible world views, beliefs, concepts and even languages, it is impossible that competing sets of paradigms can be rationally compared; there can be no mutually acceptable set of criteria by which they can be objectively and publicly judged [Kuhn 1973]. Even when

† For Kuhn's critique of Popper, see [Kuhn 1970b]. For an even stronger statement of the inherent irrationality and non-objectivity of scientific development, see *Against Method* [Feyerabend 1975] in which an anarchistic methodology of science is presented—ultimately, the only principle which holds good in scientific practice is 'anything goes'.

there may be some agreement on the type of criteria, e.g. generality, accuracy, simplicity, consistency, there will always be subjective differences as to the relative importance of these criteria:

> If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual . . . to the same decision. [Kuhn 1970a:199-200]

In Kuhn's model, the eventual 'winner' among rival paradigms will ultimately be simply that set which manages to attract the larger number of devotees, and one of the main criteria which will affect this is the ability of the new set of paradigms to give rise to a new branch of normal science which offers both a large number of new puzzles and the tools which show promise of being able to help solve them. Precisely what leads to the 'conversion' of a scientist from one scientific community to another is difficult to specify, but the process will be one of persuasion rather than incontrovertible logical demonstration or proof. It is therefore, ultimately, a subjective matter.

In anticipation of the later use for which I propose to adapt Kuhn's model, it should be emphasized here that, whatever the precise criteria are by which a scientist chooses between theories, they will be different from the criteria which an engineering designer will use to choose between several theories which might be considered for use in the design of an engineering structure.

The theory (or, more correctly, hypothesis) has an absolutely central role in the work of the scientist and in the history of scientific development:

> As [I have] repeatedly indicated, theories are, even more than laboratory instruments, the essential tools of the scientist's trade. [Kuhn 1977:208]

Other analogies might be that theories are the 'currency' of the scientists' intellectual trading economy, or the 'parameter' in terms of which scientists perceive and measure their activity. The theory or hypothesis could, thus, be seen as the historical parameter by which we measure the development of science over time. While of interest to engineering designers, theories do not have quite this central role in the history of design.

9.5 THE INVISIBILITY OF REVOLUTIONS

If scientific revolutions can now be recognized as having occurred frequently during the last two or three thousand years, there arises the question as to why they have not been 'discovered' until relatively recently. The reasons are to be found in the nature of the revolutions themselves [Kuhn 1970a:136ff]. Traditional approaches to the history of science have identified the same significant developments as those associated with revolutions, but they have been viewed simply as additions to the body of scientific knowledge. This has been shown not to be an adequate representation of how the history of scientific thought has developed.

The means by which the events of a revolution have generally been communicated to others has been through philosophical works, scientific text-books and popularizations of science in whose interest it is to present the story in an uncluttered and understandable way. It is their aim only to record the *outcome* of past revolutions and present this in a way which appeals to the practitioners of the current scientific community as a statement of their idea of 'normal science'. After a revolution, the textbooks have to be rewritten again in terms of the newly adopted language, concepts and aims (the paradigms) of the new scientific community.

The re-appraisal following a scientific revolution also includes, of course, the rewriting of the history of the subject, in terms of the new scientific community's adopted paradigms. The steps leading up to the present are reported while other events, which seem now to have led up blind alleys, are ignored. A number of heroes are identified, who are credited with certain key discoveries and their greatness is implicitly linked to the fact that they got the 'right answer' (as history has shown) and their contemporaries and antecedents got wrong answers.

The problem with this phenomenon is that it automatically leads to an incomplete and distorted view of history. It tends to imply that history is a linear development culminating in the present. It also misrepresents the work of past scientists in implying that they were working on the very scientific problem which we now credit them with having solved. This was usually not the case. Lavoisier and Priestley, for instance, were *not* looking for oxygen in their experiments; they were trying to establish the properties, such as density, of phlogiston. Similarly, Euler was not trying to find a design tool to help church architects design their columns more reliably; he was investigating the shapes which an elastic rod can take up, either when bent or when vibrating. A further trouble is that writers after one revolution are apt to be interested in different work done by a scientist from a writer after a later revolution. Thus, Coulomb's contribution to the development of the plastic design approach to masonry arches was not followed up until after the plastic design revolution in recent times (see Chapter 11).

The historian of science needs, then, to treat the records of past scientific work and the various statements of the state of the art found in print, with great care. Without any deception intended, they are likely to tell stories which do not accurately portray how the work appeared to those who were undertaking it. As we shall now see, this has been as true of the history of engineering science as it was of physics and chemistry.

10

Revolutions in engineering science

The model of historical development which Thomas Kuhn proposed for the pure sciences can also be used to help to understand and present the history of the engineering sciences. The reader will have noticed the close parallel between Kuhn's background to the development of his ideas and the introductory chapters of this book. Kuhn's model immediately avoids many of the problems relating to the history of engineering in general, and especially to the history of engineering science, which were discussed in Chapter 2.

Examples of revolutions in the engineering sciences have not perhaps been as dramatic as those in physics and chemistry, and have therefore not attracted so much contemporary written discussion and comment. To engineers, however, they have probably been more important than whether we orbit the sun or vice versa. They have fundamentally affected the ways in which people have conceived of both structures and materials and how they work, how they behave and how they fail. According to these beliefs, so different analogies and models of this behaviour have been felt, at different times in history, to be appropriate.

Periods of 'normal science' interspersed by scientific revolutions are to be found in the histories of the very building blocks of modern mathematical models of structural behaviour - the column, the arch, the beam, the truss, the shell, the pin-joint, the neutral axis, the factor of safety, the plastic hinge—as well as the particular ways in which actual materials, structures and loads can be idealized, simplified and approximated to enable physical and mathematical models to be used at all. Many of these histories go back far beyond the supposed birth of modern science in the 17th and 18th centuries, just as Newtonian mechanics has its origins in Greek science. Unfortunately the history of the engineering sciences has not attracted the same attention as the history of astronomy—there are admittedly few relevant documents and these are often in languages unfamiliar to engineers which have been ambiguously translated by scholars unfamiliar with engineering science or design (see Appendix 1, for instance). It is likely that there is still a very great deal to be found of interest to historians of engineering science in the works of Greek, mediaeval and Renaissance

philosophers. As with much discovery, a prerequisite is the belief that there is something to discover: there is too much history to rely on the chance encounter.

Many revolutions in engineering science have, of course, been associated with changes in the ways engineers have designed structures; however, as argued throughout this book, the activities of engineering science and design must not be confused. The logical independence of science and design is most clearly demonstrated by three facts:

- a revolution in engineering science is not necessarily associated with a revolution in design procedures;
- a design revolution is not necessarily associated with a scientific revolution;
- a revolution in engineering design procedures can act as the stimulus for an engineering science revolution (e.g. the plastic behaviour of steel; see Chapter 11).

By way of illustrating Kuhn's ideas applied to engineering science, two examples will be given here. They concern the constants of elasticity and theories about the bending of beams. Several other revolutions in engineering science will be met in later chapters when associated design revolutions are discussed.

10.1 THE ELASTIC CONSTANT CONTROVERSY

By the 1820s it was generally accepted, following the work of Euler and Navier, that the elastic properties of an isotropic material could be completely defined by one constant. This approach was known as the rari-constant (i.e. uni-constant) theory. At around the same time, many scientists were engaged in investigations into the nature of light and its passage through transparent solids. This involved the hypothesis that light was transmitted by means of elastic waves travelling through an isotropic elastic material known as the ether. This model depended upon the rari-constant theory and was, for instance, well able to account for the refraction of light as it passed through isotropic solids such as glass. However, another phenomenon had been observed which seemed to suggest that some materials, while appearing to be isotropic, behaved as if they were anisotropic. This was the phenomenon of birefringence or 'double refraction'—the ability of certain materials to refract light by two different amounts in different directions. Furthermore, the relative difference in refraction varied with the mechanical stress applied to the material.† The rari-constant theory was utterly unable to explain this phenomenon and the scientific community found itself in a state of (Kuhnian) crisis.

In 1839 the Cambridge mathematician George Green proposed a mathematical model for isotropic solids based upon two elastic constants. This provided the foundation for a new view of the problem and was able to account for several observed

† Actually, the difference between the principal stresses. Birefringence is the phenomenon upon which photoelasticity depends. This technique of experimental stress analysis gives the rare opportunity to investigate directly the stresses within an elastic solid without the need to measure strain mechanically. Sadly, it is often misused, perhaps because it can be used to produce visually beautiful images which are often incorrectly claimed to be 'pictures of stresses'.

phenomena, including birefringence, which the rari-constant theory had been unable to explain to the satisfaction of some investigators. The appearance of Green's paper started a controversy which was to last until the 1880s, and resulted in a significant revolution in engineering science.

This example provides an illustration from the history of engineering science which displays many of the characteristics of a revolution identified by Kuhn—the existence of scientific communities each with their paradigm theories, concepts, beliefs, experimental techniques and problems to which they devoted their attention. There is also evidence of the inability of the two communities to communicate and agree, and of their different responses to anomalies. There was even a difference in the way the two communities perceived the problem. The rari-constant community considered they were investigating whether a particular molecular model of materials could be used to derive the mathematical equations of elasticity. The other saw it rather as a question as to whether a general mathematical theory of elasticity could be used for all materials, and in particular whether the elasticity of an isotropic material could vary with direction.†

Following Navier, all the major French elasticians, including Cauchy, Poisson, Lamé, Clapéyron and St Venant, and the Austrian, Wertheim (who worked in Paris), constituted the rari-constant scientific community. The multi-constant community was made up of a British branch, Green, Stokes, Thomson (Lord Kelvin) and Maxwell, and a German branch, Neumann, Kirchhoff, Kupffer and Voigt. For several decades these two communities worked separately with their different theories, each seeking in different ways to prove their own to be 'correct'. Navier had originally developed his mathematics of elasticity for use in investigating the statical equilibrium of elastic bodies. Green, on the other hand, was interested in the reflexion and refraction of light, transmitted as elastic waves through the ether. Green's paper forced the rari-constant community to consider a new set of problems and the attention was shifted towards optics.

Disagreement between the communities arose at the most basic of levels, not only concerning the very nature of the problem but also the different assumptions made about the molecular models of matter they used—the nature of the intermolecular forces and their variation with distance and direction. Despite the interest in optics, appropriate experimentation in this field was difficult and both communities conducted mechanical experiments to gather data to support their own paradigm theories. Although both investigated the static elastic properties of materials, there were differences between the precise experimental details, such as the choice of materials, the shape of the specimens and the techniques of measurement, which the members of the rival communities held to be highly significant. The multi-constant community was able to put forward the cases of cork, jelly, rubber, and metal in the form of thin wires and plates as counter-examples in order to prove their own views and to provide data which were anomalous in terms of the French rari-constant paradigms. The French, however, could reply that these materials were not isotropic, though avoiding a clarification of precisely what they meant by isotropic. Stokes put forward a particularly strong argument against rari-constancy as a general theory.

† For a fuller discussion of the elastic constant controversy 'which plays such a large part in the history of our subject', see [Todhunter & Pearson 1886:vol.I 496ff and Timoshenko 1953:216ff].

While a perfectly elastic material might need only one constant, a viscous material, including a metal in the post-elastic (plastic) region, certainly requires two. Rari-constancy for actual specimens of metals (as opposed to ideal ones) would be untenable since it was possible to vary the point at which plastic deformation ('set') begins according to the precise means of manufacturing the specimen (e.g. heat treatment, work hardening and the creation of locked-in stresses). The reaction of the rari-constant community was again to claim that such cases were not truly isotropic.

Sometimes the criticism of the one community by members of the other transcended the conventions of scientific objectivity: Lord Kelvin, for instance, suggested that the argument of the rari-constant community was:

> essentially restricted by the arbitrary conditions imposed by mathematicians for the sake of shortening the equations by which [material] properties are expressed. [Todhunter & Pearson 1886:vol.I 364]

The rari-constant community had two principal arguments against their rivals. The first was the invalidity of tests on non-isotropic materials, mentioned above. The second was the charge that multi-constancy depended upon the materials deforming in direct proportion to stresses (i.e. being truly Hookean) which assumption could not be made about many materials.

Later, the field of experimentation grew to include the transmission of elastic vibrations, but these too led to disagreements about methods and results. The elastic constant controversy did not have a genuine resolution. No experiment could be devised by either community which would prove their case to the other. Rather, the problem disintegrated. For statical equilibrium tests on elastic metals, the multi-constant theory displaced its rival. The original concentration upon optics and vibrations in the ether, died as the electromagnetic theory of light gained currency. The rari-constant theory was generally adopted for cases involving mechanical vibrations in elastic materials, and the disagreement about the concept of isotropy was dissolved by its use being restricted to those cases for which rari-constancy could be assumed to apply.

A footnote in Todhunter's book provides a fitting conclusion to the story:

> In a letter to the editor dated September, 1885, Saint-Venant thus replied to the question of whether he continued to support the rari-constant hypothesis:
>
> > Je réponds 'oui' pour les vrais solides (supposés isotropique) commes sont les métaux ordinairement, ainsi que le marbre, le verre; mais 'non' si l'on veut absolument par un motif quelquonque que je ne conçois guère, appliquer les formules de l'élasticité au caoutchouc, aux gommes molles, aux gelées, et aux autres corps mous et élastiques, car ces corps-là ne sont que les mélanges de tissues cellulaires, de membranes élastiques, et de fluides visceux que leurs cellules contiennent.†
>
> [Todhunter & Pearson 1886:vol.I 504]

† I reply 'yes' for true solids, assumed to be isotropic, such as metals usually are, and marble and glass; but 'no' if it is particularly desired, for what reason I can hardly imagine, to apply the elastic formulae to rubber, soft gums, jellies and other soft elastic bodies, for these are nothing but mixtures of cellular tissues, of elastic membranes and of viscous fluids which fill the cells.

10.2 THE BENDING OF BEAMS

Between the work of Galileo in 1638 and the present day, many hundreds of scientists and mathematicians have devoted their attentions to the effects of loading a beam in bending. They were not, however, all working on the same problem, nor for the same reasons, nor with the same results; and, most significantly, their work did not follow a single, logical, continuous thread.

The problem of loading a beam in bending has the attractive, but deceptive, characteristic that there are only two quantities of obvious interest, the amount by which a beam will deflect under a certain load, and the maximum load which a beam can carry before failure, i.e. stiffness and strength. Furthermore, each of these quantities can be measured experimentally with great ease and it seems reasonable to assume that the stiffness and strength in bending are directly and simply related to the stiffness and strength in direct tension.

For the purposes of the present brief illustration, since the bending of beams is too enormous a subject to treat in detail, only the major trends in the historical development can be noted.† Of particular interest is the existence, over long periods of time, of separate scientific communities holding incompatible and irreconcilable beliefs concerning more than one issue.

With the benefit of hindsight it is possible to categorize various scientific communities according to their paradigm beliefs—the problems to which they devoted their attentions, and the concepts and theories in terms of which they discussed their work. In this way it is possible to highlight periods of normal engineering science and the scientific revolutions. Of particular importance among the historical data are the signs of a period of crisis which herald a revolution. These are most obvious when scientists discover a discrepancy between the results predicted by their particular theory, and the results they obtain when performing experiments to try to 'verify' it. It is the different interpretations of these 'anomalies' that can lead to the establishment of rival scientific communities. With sets of incompatible paradigm beliefs the possibility of a revolution becomes likely, whereby a new set of concepts becomes accepted in preference to the old set.

The four principal revolutions concerning the bending of beams since the 17th century have surrounded the following issues and the theoretical concepts associated with them:

- whether the major issue was seen to be flexure or fracture, and whether they were clearly distinguished;
- which notion of 'failure' was assumed;
- the distinction between the material and structural properties of a beam;
- beliefs concerning the notion of 'neutral axis'.

10.2.1 Flexure and fracture

It was Galileo who set the precedent which others were to follow, or to deviate from, in concentrating upon 'the resistance which solid bodies offer to fracture by external forces' [Galileo 1638:xxi; Kurrer 1989]. In considering fracture he assumed a brittle

† For a fuller treatment of the details of the story see, *inter alia* [Timoshenko 1953] and [Heyman 1972].

material which failed when the 'absolute resistance to fracture' (tensile strength) was reached as the two parts tended to rotate about a 'fulcrum' at the lower surface of the loaded cantilever beam [Galileo 1638:115]. From 1638 to around 1730 and again for from about 1775 to 1825, by which time 'the problem of fracture had at last been effectively solved' [Heyman 1972:101], the primary concern of engineering scientists was the strength of beams.

The elastic bending of beams (flexure) first attracted the attention of investigators interested in the shape taken by a bent beam, initially James Bernoulli (1691) and later, several others, especially Euler. This period lasted between about 1730 and 1775, during which time the interest broadened to include the vibration of beams. After a switch of interest back to fracture, bending again was the focus of attention, especially by Navier and St Venant, between about 1825 and 1864, by which time 'the problem of the beam had been essentially solved' [Heyman 1972:109]. During this period there was also a diversification of interest to study of the transmission of waves in elastic media (see §10.1).

It is of particular note that most of the workers interested in the flexure of beams were also pursuing their desires to find problems which could make use of Leibnitz's infinitesimal calculus. This followed from the work in Basel of the Bernoulli family and Euler in the early 18th century. Even as late as 1773, Coulomb entitled his famous *Essai* 'sur une application des règles de Maximis & Minimis . . . '. Such paradigm articulation is characteristic of periods of normal science.

Although the various investigators were usually explicit about whether they were considering flexure or fracture, during the entire period up to Navier (1820s), it was quite usual for them to fail to see any connection between the ways of looking at both. The confusion principally concerned the inability to think independently about stiffness and strength and the mode of failure (brittle or ductile) associated with the ultimate strength.† Thus Coulomb, following the precedent of others, treats the cases of stone and timber differently and 'offers no hint that he might have grasped that the elastic formula could be used for stone before fracture' [Heyman 1972:98].

Faced with the contradiction of applying elastic formulae to calculate ultimate loads for beams of all materials irrespective of the ductile behaviour of some of them, Navier clearly states that materials can only be treated similarly up to the limit of their elasticity [Timoshenko 1953:74]. In doing this he heralded the revolution, based upon a new concept of failure, which finally allowed the clear distinction between strength, stiffness and mode of failure, and was to fix theoretical and experimental paradigms right up to the present day.

10.2.2 Notions of 'failure'

During much of the 18th and 19th centuries there were two rival views as to the precise nature of failure in a material and this led to two rival scientific communities. The origins of this dichotomy lay back in the 17th century with Mariotte and Leibnitz who postulated that the criterion for fracture of a material was reaching a limiting value of strain. This belief continued in France beyond the middle of the 19th century. During the same period in Britain, however, it was universally believed that fracture occurred when a certain stress was reached [Timoshenko 1953:89,131ff]). In the early

† The same confusions are, in fact, still common amongst present day engineering students.

19th century Navier drew attention away from the idea of fracture as the failure criterion by concentrating upon the yield point of a material—the limit of its elastic behaviour. Initially, interest in the new criterion for failure followed the paradigms existing on each side of the English Channel—limiting stress to the North and limiting strain to the South. Later, however, there was increasing dissatisfaction with the accuracy of the predictions of the material strength (complicated by the discovery of metal fatigue in the 1840s). This led to the proposing of many other possible criteria, including limiting stress, strain and strain energy, and shear stress, strain and strain energy. What began as two possible criteria for failure, had multiplied to half a dozen or more, with no apparent means of choosing between them. Even as recently as 1931, a review of the current theories of failure, and the experimental evidence in favour of each, could only conclude:

> It will be apparent from the preceding evidence that the correctness of any one of the theories outlined above is yet to be definitely established, and that the criterion for elastic breakdown is still an open question.† [Salmon 1931:485-499]

A recent proposal recommends the adoption of six different definitions of yield and three sets of criteria for yield, each of which has been 'verified' under particular conditions in laboratory experiments, but no one of which is universally applicable [Haythornthwaite 1968:203-209].

The original, apparently simple problem of relating the strength and stiffness of a beam to the strength and stiffness of the material of which it was made, had been solved by St Venant and others in the 1860s, to the extent that a model was proposed which, given accurate data about a particular material, enabled the strength and stiffness of a beam to be predicted. In another sense, however, this solution simply begged questions concerning the measurement of stresses and reliability of such data, given that stress cannot be measured directly (internal stresses can only be computed from the measurement of external strains and the use of the very theories of bending which were being investigated).

A final resolution to the original problem of the failure of a beam has now been abandoned. The question as to whether limiting stress or strain is the key criterion has been replaced by a host of new questions about the sub-microscopic structure and behaviour of the material itself. Several new revolutions in materials science came out of the chaos of the late 19th century. For yield in metals, the failure of the material is now considered in terms of dislocations at the atomic level in the metal crystals (due to Taylor in 1934); and fracture is explained in terms of stress concentrations around the roots of small cracks (due to Griffith in 1920). The failure of other materials, such as timber, is also generally explained in terms of their atomic and microstructure (see [Gordon 1976]). Meanwhile, engineering designers continue, for the most part, to make use of the out-of-date 19th-century theories of elasticity and strength of materials. Recently, the plastic revolution offered yet another approach to the idea of failure which has avoided the whole controversy which dominated the interests of engineering scientists for some 200 years (see Chapter 11).

† The same was conclusion was reported by [Roś & Eichinger 1926]

10.2.3 Notions of 'structure' and 'material'

At the very beginning, Galileo had treated two materials (wood and stone) as if they behaved differently in bending both up to and at fracture. This belief, which resulted in considerable confusion, persisted universally until well after 1800. The significance of the 19th-century work of Navier, Hodgkinson and St Venant was that it finally separated the notions of 'structure' and 'material' and allowed their independent consideration.

A confusion of a different kind was made by Mariotte who attempted to 'verify' his theoretical prediction of the fracture strength of a rectangular beam (Galileo's problem) by testing rods of circular cross-section. This type of confusion (though not this particular one) was common until the geometrical notion of second moment of area of a beam section (I) had been finally distinguished from the elastic properties of the material (E). Euler had successfully used the product of these two quantities (EI) in 1727 (when he was 19) and had conceived of their separability. However, even Young, the supposed father of the modulus named after him (E), had still failed, in 1807, to understand that elasticity was a property of a material, rather than of a body of material [Truesdell 1968:218]. E and I were still being treated together as late as 1826 in Navier's textbook.

It is interesting to note an earlier conceptual revolution which anticipated the eventual complete distinction between a property of a material and of a body of material by some 200 years. It is likely that it was Galileo who finally fixed our modern notion of tensile strength as a material property, measured as a force acting on a certain cross-sectional area of material, i.e. stress. He made use of this notion in arguing against the view held by 'many very intelligent people' that the strength of a rope diminishes with its length [Galileo 1638:121–122]: he distinguished between a weakness due to a smaller cross-section and one due to an inferior quality of fibre. Thus he was able to demonstrate that a longer rope was indeed likely to be weaker than a shorter one, but only because it was more likely to contain a manufacturing flaw or section of inferior material. The strength of the material itself did indeed vary, but not because of its length.

10.2.4 The neutral axis

Superimposed on the demarcation between scientific communities interested respectively in fracture and flexure, and holding the different notions of failure, there was also the question as to precisely which mathematical model was adopted for the distribution of stresses within the material of the cross-section being bent. This issue is usually simplified to the question of the 'position of the neutral axis'. Such a simplification is misleading, in particular because the term 'neutral axis' was first popularized by Tredgold in 1822 [Tredgold 1822:53] by which time the problem of bending had been virtually solved.

The modern meaning of neutral axis comprises two interpretations - the supposed location of the plane of fibres which is neither stretched nor compressed, and the location of the virtual axis about which the molecules of the beam can be thought of as rotating. The first of these interpretations is a logical conclusion of the mathematical model, in terms of statics, of a beam in bending. Since it is impossible to verify or falsify the supposed position of the axis, it does not matter what position it is given; so long as the rules of statics are followed, it depends only on the stress/strain relation-

ship of the material. In fact, St Venant (who 'solved' the problem) proposed a generalized form of equation which would allow the neutral axis to be placed at a position appropriate to *any* material stress/strain relationship [Timoshenko 1953:135ff]. Mariotte had been the first (in the 1670s) to suppose the position of the neutral axis to be irrelevant, though he later changed this view when concerned with fracture (Girard and Navier did the same c. 1800). James Bernoulli (1694), on the other hand, was concerned only with the proportionality of bending moment and radius of curvature in the elastic region, and this result is truly independent of the position of the neutral axis. His work led to the establishment of the scientific community centred in Basel, which made use of Leibnitz's new calculus, particularly in its study of elasticity and vibration during the period 1720–1780.

The second interpretation of neutral axis is an attempt to visualize what is actually going on in the beam—how the structure is behaving. It was this approach which led Galileo to suppose that when his brittle cantilever broke, it did so by rotating about a line on the bottom face. It is often implied that in doing so he mistakenly placed the neutral axis at the lower surface. In fact, Galileo, who uses the word 'fulcrum', is in one sense, entirely accurate—virtually all the fibres of a brittle material loaded in bending do fail in tension. Without the aid of very high-speed photography and a modern understanding of fracture mechanics, it was both intuitive and entirely reasonable to assume that the entire section rotates about the lower edge. Parent even referred to the 'axis of breaking" [Truesdell 1960:112]. It was the clear distinction between this idea of neutral axis and the statical one, which logically required the statical equilibrium of each part of the beam at all times, which was to hinder progress in the understanding of bending for so long.

Over the next 167 years following Galileo, Mariotte, Leibnitz, Bernoulli, Lahire, Varignon, Parent, Bélidor, Coulomb, Girard and Robison all went into print on the subject of the fracture strength of beams placing the 'fulcrum' at the lower surface. And yet as early as 1685, Mariotte had realized that, in terms of horizontal statical equilibrium, such a mechanism was untenable; for Galileo's 'fulcrum' he substituted the *axe d'equilibre*. Parent (1713), Belgrado (1748), Coulomb (1773) and Barlow (1818) also realized the logical contradiction and modified their earlier Galilean ideas. It was, however, not until Hodgkinson and Navier (in the 1820s) that the intuitive interpretation of the fracture of brittle materials in bending was finally rejected.

The 180-year period between Galileo and Navier indicate just how confusing it was to have two, apparently contradictory, notions of what is now known as the neutral axis—the fulcrum and the axis of equilibrium. It was very difficult to argue, in the face of the evidence of brittle fracture, that half the cross-section of a beam acts in compression. As early as 1713 Parent had anticipated a solution to the paradox in suggesting that the neutral axis could move from its initial central position to being at the compressive face at the moment of fracture. The modern view in terms of fracture mechanics would also propose that the neutral axis must move, but this would only occur at fracture, not while the bending was still elastic. Parent had still not clearly differentiated between what we now regard as the separate problems of flexure and fracture.

Only with experiments on non-brittle materials, such as those by Barlow on timber in 1817, was it eventually demonstrated that at least part of the cross-section of the beam had not failed in tension but by crushing and shear of the fibres in the part of

the beam suffering compression. The confusion was only resolved when the notions of flexure, fracture and brittleness were finally and completely separated.

10.2.5 Explanations of anomaly

The revolutions associated with the changes of concepts in terms of which the behaviour of engineering structures has been described, tend to be rendered invisible by the rewriting of history in terms of the newer concepts—a characteristic of revolutions. A major source of evidence for scientific revolutions is to be found in the observation of 'anomalies', as Kuhn calls them, or discrepancies between theory and practice.

By the early 18th century, both Parent (1713) and Bulffinger (1729):

> were moving towards the conclusion that the calculation of the bending strength of a beam was not necessarily a simple matter. [Heyman 1972:95]

An early example, in the history of the bending of beams, of explanations of observed anomalies is found in Mariotte's 'excuse' that his measured strength of a beam was some 10% higher than his mathematical model predicted because of a 'time effect' [Timoshenko 1953:23]. Until the clear separation of the problems of flexure and fracture it was widely assumed that different mathematical models of bending applied to different materials according to their material properties—either brittle and stiff, like stone, or non-brittle and less stiff, like wood. This implicit explanation of anomalous experimental results reached a more sophisticated level in the supposition that the behaviour of an elastic material was different in tension and compression, first suggested by Parent (1713) and taken to its limit by St Venant (1864) in allowing for the neutral axis to be placed anywhere by assuming appropriate elastic behaviour in tension and compression [Heyman 1972:96–105].

A common, but from the rigorous theoretical point of view, unacceptable reaction to the observation of discrepancies between the predictions of mathematical models and experimental findings, has been the introduction of empirical constants to achieve a closer fit between predicted and actual results. Hodgkinson was just one of many experimenters who proposed models in a *form* which was theoretically justifiable, but which he then modified using an empirical constant to achieve a closer match between 'theory' and 'practice'. These constants were not, however, genuine concepts of engineering science since they had no correspondence with a proposed real-world property of either the material or the structural form under test [Heyman 1972:101]. (The history of predicting the buckling strength of columns has been even more populated by such empirical constants: see [Salmon 1921].) As argued in §6.1, this expedient has been of much greater benefit to engineering designers than to engineering scientists and frequently gets incorporated in the designers' 'factors of safety', a concept of no interest to the engineering scientist.

This and the previous chapter have sought to introduce the historiographic technique of looking at the development of engineering science in the same manner that Kuhn has looked at the history of physics and chemistry, in terms of scientific communities with their own paradigm beliefs, theories and concepts. Such scientific communities are born and die as periods of radical change (revolutions) interrupt periods of rela-

tively calm development (normal science). In a revolution the whole approach to a subject changes, especially the concepts and language used to describe the problems of concern, to an approach which is logically incompatible with its predecessor. Kuhn's model has been introduced, firstly, to illustrate an new approach to the history of engineering science and as a proposed solution to, or avoidance of, the problems of looking at the history in more conventional ways (see Chapter 2). Secondly, having introduced Kuhn's model, it can now be adapted for use in treating the history of engineering design.

11

The plastic design revolution

Between about 1930 and 1955, the design procedures for steel-framed buildings underwent a radical reappraisal which few people would hesitate to describe as revolutionary—in many ways the changes were the most significant since the birth of the metal portal frame in the 1850s. The outcome of this period of engineering history is already familiar to most structural engineers—'plastic design procedures' have now replaced elastic methods when designing certain steel and, more recently, reinforced concrete structures.

A superficial look at these changes would suggest that, in some way, the new methods are 'right' and they have 'obviously' replaced the other methods which have been found to be 'wrong'. This is a common attitude to design practice in the past, implying that present practice is better than that of our ancestors. However, it overlooks the significance of what the changes really were and why they came about, just as it is now only too easy to look back at the 'discovery' that heat was a form of energy and not a substance called 'caloric', as if it had been merely a matter of getting the right answer to a clue in a sort of cosmic crossword puzzle.

As the examples of the previous two chapters have shown, Kuhn has offered an alternative way of looking at the history of science which avoids the problems of dealing with history as if it were simply a series of sequential events each of which ought to be able to be discovered, if only we knew where it had been written down. In addition to providing a way of interpreting events in history, he has also given us an understanding of how we can assess progress in a field of study. In essence, Kuhn has drawn our attention to the influence of certain psychological issues on our history—the way we, and people in the past, see the world about us is not an objective matter, for 'seeing' requires interpretation and this, in turn, is dependent on the language and concepts we use, our beliefs, our intentions and the aspirations of both individuals and communities. These issues affect not only our morals, but also apparently objective activities such as science. A consequence of this state of affairs is that there is a profound upheaval when one set of beliefs (Kuhn's paradigms) is challenged

by newer set, and engineering design is as susceptible to such events as politics and science.

The cycle of events during the period in the history of steel frame design, between the late 1920s and mid-1950s, display all the classic symptoms of a Kuhnian 'revolution'—there had been a long and stable period of 'normal engineering design' employing design procedures based on the elastic behaviour of materials. These and other paradigms were the accepted beliefs of all the established 'design communities'. Work in this environment gradually gave rise to more and more 'anomalies' which eventually became so disturbing that some people came to perceive a state of 'crisis'. At first, a solution was sought in terms of the old (elastic) paradigms; this attempt at 'paradigm articulation' failed.

A radically different approach, which took account of steel's post-elastic (plastic) behaviour, was proposed and tested. The 'problem' now *looked different*. It was described in terms of new concepts which formed part of a new set of paradigms. In this manner the anomalies were solved, or rather, disappeared. The new type of design procedure slowly gained acceptance with more and more designers, who came to constitute a rival design community. The paradigms of the 'elastic design community' were discarded (by some) in favour of an alternative set of paradigms incompatible with the previous set. Development of both the new type of design procedure ('plastic design'), and the branch of engineering science to which it had drawn attention, now progresses in ways and directions literally inconceivable in terms of the pre-revolutionary paradigms.

The story is also particularly interesting because it provides an excellent example of profound changes to design methods which were due neither to a new invention or discovery in engineering technology, nor to a catastrophic accident involving loss of life, nor to the recent 'verification' of a scientific hypothesis, all of which are popularly supposed to be the mechanism by which engineering progresses.

We are fortunate that the dramatic story of this important period in the development of structural engineering design procedures is both well-documented and recent—indeed, still within living memory. We are also fortunate that one of the leaders of the research projects, J. F. Baker (later, Professor Sir John) was an exceptional writer who not only told the story with the skill of a novelist, but also included far more of his personal observations and feelings about the work in which he was immersed than is usually recommended for reports of academic and 'scientific' work.

We begin the story of the 'plastic design revolution',† only the highlights of which are presented here—the full story deserves a book of its own, with a look at the paradigms of the previously established 'elastic design community'.

11.1 ELASTIC DESIGN PROCEDURES

The use of the mathematical theory of elasticity in structural engineering design procedures had been common since the early 19th century. By the 1840s engineering

† Recently the phrase 'plastic design' has tended to be replaced by the more general idea of 'limit state' design—itself a step in the story of the revolution. I preserve the use of *plastic* design since this was how Baker and others conceived of their new approach to frame design; it also contrasts conveniently with 'elastic' design.

scientists had thoroughly tested both elementary and more sophisticated theories of elastic bending. By the late 19th century they had also established that there were several ways of defining elastic failure according to precisely which yield criterion was accepted. In other words, although several parts of the full behaviour of a material up to failure were understood, there was, as yet, no general theory which would reliably relate either the yield or the ultimate strength of an actual beam to the corresponding strength of a laboratory sample of the material in simple tension.

Given this state of affairs, the design procedures of the late 19th century consisted of two sets of 'grounded rules': one concerned the ultimate strength of beams; the other, the behaviour of beams and columns within the elastic range.

The design procedures relating to the ultimate strength of beams were not of a new type. In the absence of a general theory of strength, they were still based upon grounded rules which had their origins in the empirical laws of the type which Galileo had suggested (in 1638) relating the strength of different sized beams of similar shape to the geometry of the cross-section and the length of the span. A large number of these, relating to both beams and columns of many materials, had already been long established (see, for example, [Musschenbroek 1729; Tredgold 1822]). Although the grounded rules were not based upon a fully tested scientific and mathematical theory of stength, they did rest upon the almost intuitive explanation of strength in terms of the amount and the disposition of material. The grounded rules comprised formulae, containing key dimensions and empirical constants which varied according to the particular material, and had been established in strength tests.

Thus, the design procedure for the case when beam strength was the principal criterion, was able to provide a justification of the results partially in terms of tests to establish empirical constants and partially in terms of grounded rules, whose bases were empirical laws, and which contained an intuitive explanatory relationship between a beam's strength and its size. Throughout the 18th and early 19th centuries there had been two approaches to the strength of beams—the 'absolute strength' and the 'relative strength'. The former was largely the concern of scientists aiming to relate the strength of a beam to the material strength; the latter was of interest to designers who were interested in the strength of a certain beam relative to one of different dimensions, and possibly of a different material. Relative strength could be encapsulated in a grounded rule which incorporated the variation of strength with geometry (length, breadth and the square of the depth for a rectangular beam) and the other variations, such as material, in an empirical constant. Relative strength did not rely on figures for tensile strength or on a theory of bending.

By the late 19th century, the power of the justification had been further increased by incorporating two paradigm concepts borrowed from the mathematical theory of elasticity, which was then held in extremely high regard. The geometric information concerning the beam section was presented in the formulae in terms of the second moment of area, I, and the maximum stress which the material could withstand in tension (mirroring the idea of the maximum stress the material could sustain before 'elastic failure', i.e. yield). Subsequent developments were to show that neither of these concepts has any place in relating the dimensions of a beam and the ultimate tensile strength of the material to the strength of a beam made of a ductile material. That such design procedures were both effective and safe was entirely due to the large quantity of reliable tests used to determine the empirical constants, and to the large

safety factors used. In practice, members made of ductile materials such as wrought iron and steel, were by this time nearly always designed according to the more popular design procedures based only upon behaviour in the elastic range.

For design procedures based upon structural behaviour in the elastic range, a lot of reliable data was available for the two fundamental paradigm cases—the beam and the column. Each of these incorporated other paradigm beliefs originating in the mathematical concepts of the theory of elasticity and strength of materials. A beam was seen as resting on two or more supports. Its extremities could be free to rotate or fixed (encastred) and the rotation of the beam at intermediate supports could be free or elastically restrained. It was also possible to cater for differential sinking of the supports. A column, or stanchion, was seen as being loaded axially with the additional possibility of variously restrained ends and eccentricity of load. The material of a beam or column was assumed to be perfectly elastic in both compression and tension up to an elastic limit, beyond which the assumptions of the theories of elasticity were no longer valid. For both columns and beams safety was ensured by the specification of a level of stress, the working stress, which should be exceeded in no part of the structure and which was lower than the elastic limit of the material by a margin referred to as the 'factor of safety' (this term had been inherited from earlier design procedures in the 17th and 18th centuries when ultimate strength had always been the design criterion).

In investigating the theories of elasticity, 19th-century engineering scientists had extensively studied cases which were both extremely simple and highly controlled. However, in even a relatively simple actual structure with a considerable degree of redundancy (which all real structures have), the use of elastic mathematical models combined with the empirical data giving the limits of their application, led to extremely tedious and complex equations which, even today, would be difficult to solve. The result was that designers needed to make drastic simplifications and idealizations of actual structures in order to create mathematical models of loads, materials and the structure which were simple enough to manipulate.

So it was in the early part of this century, that the design procedures for steel-frame buildings not only incorporated the general paradigms of elastic structural analysis, but also contained the simplifications and idealizations (further discussed below), each one of which, taken in isolation, was perhaps not unreasonable. Together these generally accepted approximations constituted a further set of paradigm beliefs about the structures being designed.

For the purposes of the design procedure, a steel-frame building was seen as a grid of vertical stanchions which were connected by horizontal beams. Each beam was treated in isolation and usually taken to be a single span between either simple or semi-rigid supports. Each stanchion was treated alone as reaching from one floor to the next, restrained at each end to some degree, and with the vertical load being axial but with a certain eccentricity. The 'degrees of end-fixity' and the load eccentricity were chosen on the basis of the designer's judgement. The worst situation to be designed against was when each floor was fully loaded. At other stages in the design procedure, for instance when considering the wind load on the whole building, the joints between stanchions and beams were treated as fully rigid (see [Baker 1932:3 and 1954:1]).

As well as the above paradigms, in terms of which frame buildings were described

and thought of, there were also the ones relating to elastic design procedures in general. These set the maximum permissible working stress of steel to be 9 tons/in^2, which meant a factor of safety of 2 against elastic failure at the minimum guaranteed yield stress for steel of 18 tons/in^2.

The design procedures were able to offer justification of their results in being supported by grounded rules—rules based on formulae derived from experimental testing of the mathematical theories of elastic behaviour of beams and columns, and the elastic properties of materials. These were used in conjunction with empirically derived constants to cater for the end-fixities of the beams and columns and the various discrepancies between the simple formulae and empirical results. As with the case of the design procedures for the strength of beams, the elastic design procedures were given greater powers of justification by virtue of being expressed in a form similar to that used by the theoretical elasticians and the engineering scientists in their meticulous laboratory work.

By 1930, these design procedures had worked successfully for more than fifty years and, in Britain at least, there had been not a single recorded instance of a collapse of a frame building. As will be discussed below, this fact was due rather more to their conservatism and the lessons of precedent than to anything more 'scientific' or 'theoretical'.

11.2 ANOMALY

The use of the term 'anomaly' to refer to a stage of a design revolution needs a little clarification. In discussing scientific revolutions, Kuhn uses 'anomaly' to describe a discrepancy between 'a paradigm-induced expectation' and what is actually observed [Kuhn 1970a:52–53]. In the design of engineering structures, the direct analogy of scientific anomaly would be a failure of some kind which had not been foreseen by the designer. A failure need not, however, be as dramatic as a structural collapse. Any failure of a building to perform to the client's or architect's requirements can constitute an anomaly. It might, indeed, relate to a building's structure, such as a case of inadequate stiffness or poor resistance to earthquakes. It could also, however, relate to the process of constructing a building or to its cost. It could even concern rather less objective matters, such as the economy with which material has been used or a structure's aesthetic qualities—both technical and visual.

It is in this last category that a particularly important type of anomaly is to be found among the 'less objective matters'. It may simply be believed or felt, without the support of objective evidence, that a design procedure has failed to yield a design which cannot be improved upon. This would constitute a discrepancy between expectations arising out of the current paradigms and the design as actually achieved.

In the construction industry, this type of phenomenon has been particularly important because of the very few occasions when it is possible to carry out real-life tests on a proposed improvement to a design procedure. The anomaly represents a discrepancy between how a structure is designed and, on the basis of someone's understanding and beliefs about such structures, how it could be designed more safely or cheaply or could be constructed more easily. This process is clearly an intellectual one, and lies at the very heart of the creative part of engineering design. As might be

expected, therefore, it is by means of this intellectual process that developments can take place without the 'need' for catastrophic accidents—the engineer's equivalent of the physicist's 'thought experiment'. Examples of the catastrophic type of anomaly are not uncommon and several examples will be mentioned in the next chapter. The anomaly observed in the plastic design revolution is an example of the other kind.

The London County Council had drawn up regulations for the construction of steel-frame buildings in 1909. They continued to be used without change for some 23 years. However:

> long before the expiration of that period, the development in the manufacture of steel and the advance in the technique of steel-work made engineers feel that the regulations were unduly restrictive and that they did not allow full advantage to be taken of the excellent qualities which steel possesses as a material for building construction. [Baker 1935/6:127]

And so, in 1929, the British Steelwork Association, with Government support, set up the Steel Structures Research Committee (SSRC). Its brief was twofold:

> 1. To review present methods and regulations for the design of steel structures, including bridges.
> 2. To investigate the application of modern theories to the design of steel structures, including bridges, and to make recommendations for the translation to practice of such of the results as would appear to lead to more efficient and economical design. [Baker 1935/6:128]

Behind this statement of the brief, however, lay many other matters (anomalies) which had caused a number of engineers of the period considerable concern. Several of these concerned the empiricism of the current design procedures:

> Designers of steel frame buildings are recognizing more and more clearly that their methods of calculating the stresses in the members of the frames are largely empirical . . . .
>
> It is common practice, when considering the effect of floor loads, to assume that all the beams are simply supported on brackets attached to the faces of stanchions. When wind loads have to be borne, however, this assumption is considered to be unsatisfactory, and it is then customary to imagine a perfectly rigid connection between each beam and stanchion . . . Though apparently leading to simple methods of calculation, these patently contradictory assumptions, by failing to give a true picture of the stress distribution in the frame, actually give rise to fresh problems, such as the eccentricity of loading and the 'equivalent length' of a stanchion, which have no solutions. [Baker 1932:3]

> [The structural engineer] knew that some account had to be taken of eccentricity of loading, but one of the most disputed points in the method of design was what to assume. [Baker 1954:1]

> It seemed probable . . . that [the method of design] was in the nature of an empirical formula in which the numerical factors, appearing in the regulations under the guise of superimposed loads, working stresses, eccentricities and end-fixity, were merely constants justified by experience. [Baker 1954:2]

> In the present method of design it is assumed, when estimating stresses in members, that all members are loaded. This may not produce the most rigorous stress conditions. [Baker 1935/6:136]

In summary, the SSRC confirmed that the design procedures of the time were irrational. This had a most important consequence:

> [The] review of existing regulations left a strong impression that the method of design of steel framed buildings in common use had no firm rational basis, and that the wide range of constants representing loads and stresses made it difficult to justify even as empirical. It was clear that no advance could be made until the real behaviour of this form of structure under load was understood. [Baker 1954:6]

> Simplified assumptions must always be made by the designer, but if those assumptions are so sweeping that the true behaviour of the frame is disguised, then economy of material and evolution of the method of construction are impossible. [Baker 1935/6:179]

The first response to this awareness of problems with the existing design procedures was, naturally, to persist with the elastic design philosophy as a route to avoiding the contradictions and to breaking down the barriers to progress. The first step was an immediate response to the perceived emergency:

> So unsatisfactory . . . were the rules governing the use of steel in buildings that it was felt desirable to draw up recommendations for a Code of Practice [later BS 449], based on . . . the available knowledge and experience of practising engineers. [Baker 1935/6:128]

The second step was to discover how framed structures actually did behave; this was the major work of the SSRC. The results were startling:

> The investigations . . . show that the stress calculations made in design today give a very faulty representation of the distribution of stress in a frame. What is more important, however, they disprove the usual assumption that the worst conditions are provided for each member when every member carries its full load. [Baker 1935/6:178-9]

Particular results from test structures and actual buildings exposed severe problems, for instance, bending stresses in stanchions 'as much as nine times those which would be estimated by the existing methods of design' and an 'equivalent eccentricity of each connection . . . approximately twice that which would have resulted from the usual assumptions' [Baker 1935/6: 156,147]. While it is conceivable that such differences could have been patched over by changes to the empirical constants, some other findings were so utterly inexplicable in terms of the paradigms of the current design procedures that warning was given of the need for a more radical solution:

> As the [beam] connection was to the web of the stanchion [in one of the test buildings] the eccentricity would have been taken by many designers to be zero, and by the conservative designer as not more than 2 inches. In actual fact the 'equivalent eccenticity' . . . was 9.3 inches . . . The maximum bending stress observed at the foot of each stanchion was greater [by 14%] than would have been predicted on the assumption of perfectly rigid joints [the logical maximum!] . . . .
>
> In no part of the range does there appear [for connectors between beams and stanchions] to be a true linear relation between moment and rotation; some inelastic deformation occurs under the smallest moment. [Baker 1935/6:140,144,170]

The final step taken by the SSRC in response to their findings was to propose new, rational elastic design procedures:

> The difficulties arising from [the] artificial and insoluble problems [of the current design procedures] can only be overcome by using, from the beginning, a more accurate method of calculation based on assumptions closely representing the conditions found in an actual frame . . . [while being aware that] unfortunately all the more accurate methods evolved in the past have been much too laborious for use in the drawing office. [Baker 1932:3]

And so, a new design procedure was proposed by the SSRC. In anticipation of it being found too complex by practising design engineers, a simpler version was also appended. The reception was cool:

> Whatever criticism could be levelled at the method of design which had held the field for nearly fifty years, it certainly had the merit of simplicity; in fact it would be difficult to imagine anything simpler. By comparison, the method of stress calculation and the design procedure recommended by the Committee were laborious. [Baker 1954:183]

Not only did the new design procedure mean harder work in the design office, neither did it lead to economy of material. A further simplification of the methods, returning to the style of the previous empirical methods, was put on trial in several structural design offices between 1937 and 1940 but even this failed to gain acceptance. Further steps in the same direction were curtailed, perhaps fortuitously, by the Second World War.

11.3 CRISIS

The Steel Structures Research Committee had been set up to conduct investigations into certain 'anomalies' which had been identified in the current elastic design procedures and to formulate a new, more rational design code which would better exploit the structural properties of steel. By 1940 it was apparent that it had failed to achieve this goal. The SSRC had discovered that things were, in fact, much worse than previously suspected—values of the principal parameter (stress) used in the elastic design codes which were calculated as part of the routine justification of a proposed design, were found to differ considerably from values found in actual structures. The assumptions behind the procedures were also found to be even more irrational and contradictory than had previously been realized.

Thus, while the elastic design procedures were still able to help with the description and specification of safe stuctural designs, their power to justify the designs was severely weakened. Their credibility had been challenged and found wanting. The attempts to repair this damage by proposing more rational elastic design procedures had been rejected by designers as being too complex and they contributed little or nothing towards material and cost savings. A rather desperate state of crisis existed, particularly amongst the members of the SSRC:

> While there is no doubt that the investigations made by the SSRC provide the only sure guide to the development of a sound method of design based on elastic conditions, they make it clear that the limits of economy cannot be reached in this way. [Baker 1954:190]

> The investigators who had carried the main burden of the Committee's work realized . . . that the Recommendations would apply to only a small part of [a steel-frame

cinema under construction in Bristol c. 1935]. What is more, they knew . . . that the elastic behaviour of [the steel frame] would be so complex and variable that no practicable general rational method of design of the SSRC's type could be derived. They were equally confident that the engineers responsible for this particular structure had been dissatisfied with many of the assumptions and approximations forced on them by the orthodox method of design and would have been non-plussed by any change in method of fabrication, such as the use of welded connexions then becoming possible, which might alter the character of the structure.

This was an intolerable position. It was hard to believe that no better general tool could be devised than the orthodox method of the Code of Practice [BS 449]. If this were so then the rivetted and bolted structure had reached its final and most economical form and the community would be denied indefinitely the full benefit of the rapidly developing technique of joining members by welding. Might it not be so that the whole basis of elastic design was at fault and that the path which designers had been following for nearly a century was nothing more than a blind alley? [Baker *et al.* 1956:1]

In addition to the above problems associated with the elastic design procedures, there were two further areas of concern:

(1) Direct design using elastic methods was impossible. A real structure had to be proposed in considerable detail before its performance could be analysed.
(2) Elastic structural analysis of real structures yielded very unreliable data concerning actual stresses, and absolutely no data at all relating to the ultimate safety of a structure against collapse.

It was also already well known that, beyond the limit set by the elastic failure (yield) of steel, there was a reserve of strength which current design procedures were unable to take into account. This knowledge had been perhaps the greatest contributor to the belief that most steel structures were overdesigned—the very belief which had prompted the setting up of the SSRC. In essence, in terms of the paradigms of elastic design procedures, it was impossible even to conceive of a steel structure actually collapsing, let alone to estimate the loads which would bring this about. This state of crisis had now led Baker to suggest 'that the whole basis of elastic design was at fault' and thereby to propose looking at the whole issue in a radically new way.

11.4 REVOLUTION

In 1935 Baker had presented the results of the work of the SSRC to the Insitution of Civil Engineers in a paper entitled 'The Rational Design of Steel Building Frames' [Baker 1935/6]. In the discussion which followed this paper a design engineer in the audience indicated that he, too, had perceived the fundamental inadequacies of elastic design procedures—in fact, he was little less than totally dismissive of the work of the SSRC:

Why was it not easily possible to arrive at the ultimate strength of the complete structure? It was easy to arrive at the ultimate strength of any isolated member . . . by merely loading it to destruction on a testing machine . . . .

[It was] a serious objection that the experiments were limited to stresses which were well within the elastic limit. The range of stresses discussed in general, therefore, was

> less than half the total range. It was necessary to know the utimate strains of the structure . . . .
>
> What was required was not a modification of the existing theory of elastic structures but a completely new theory based on the conditions in the total structure at its ultimate load. [Manning, in the discussion of Baker 1935/6:217,219,220]

To these points Baker replied that:

> ultimate strength and higher working stresses leading to the region beyond the yield point might prove of the greatest importance in steel frame design, as they were, although probably not so recognized generally, in the case of reinforced concrete structures. [Baker 1935/6:228]

It was the development of design procedures based upon 'ultimate strength and . . . the region beyond the yield point' which was to occupy Baker and others for the next 15 years. The details of this work are not here of primary concern (for a large part of the story see [Baker *et al.* 1956]). More to the point are those matters relating to the establishment of the new paradigms which came to characterize the radically new approach of the nascent design community.

Baker's first task was to find out what was known of the behaviour of materials beyond their elastic limit. The search led to a reappraisal of history typical of the immediate post-revolutionary phase. In fact, a considerable body of work was discovered which had hitherto been largely overlooked, since it could find no place amongst the paradigms of the currently orthodox elastic design procedures. References to the importance of considering the behaviour of materials beyond yield was traced back as far as 1892:

> There exists no adequate mathematical theory of set or of after-strain, or in fact of any of the phenomena exhibited by materials strained beyond their elastic limits . . . Yet it is imperatively necessary that effects which cannot be calculated exactly should be taken into account in constructions, and it is in this sense that elastic theory is at this time behind engineering practice. [Love 1892:vol.1:109] cited in [Baker *et al.* 1956:5]

It also turned out that Baker and his colleagues had not been the first to conceive of incorporating the plastic behaviour of steel into design procedures. By one of those, perhaps not-so-coincidental coincidences of history, the year of the publication of the Recommendations of the SSRC also saw a conference (in Germany) on plasticity, at which was presented one paper tracing the work towards the establishment of plastic design procedures back to 1914 [Kazinczy 1939].

The six procedures Kazinczy lists indicate, however, that he had not yet completely transcended the paradigms of the elastic procedures. They consist mainly in extending these procedures up to collapse by the introduction of an empirical constant by which amount the elastic failure (yield) stress must be 'raised' when considering ultimate strengths of structures. This constant had to be determined experimentally for each type of structure (and material, of course).

Despite the discovery of this previous work which had established some of the fundamental concepts of plasticity, Baker's primary aim was to establish a fully developed design procedure. The first step was to investigate the possibility of predicting the collapse loads of some simple structures—predictions of a type which were so inaccurate when using elastic procedures. Baker's preliminary tests:

> showed that portals subjected to vertical loads had a great reserve of strength beyond the point at which yield was first developed, and that collapse did not occur until a mechanism had been formed by the development of three plastic hinges. It was also possible to deduce from them evidence of the first importance that the collapse load was independent of the inevitable practical imperfections, such as lack of complete rigidity of connexions and spreading of stanchion bases. [These had made] the development of a rational design method based on elastic behaviour so difficult a task. [Baker *et al.* 1956:12]

Thus, the basis for the new design procedures was established and the main part of the revolution was over. It is interesting to note when reading Baker's book of 1956 that there is a subtle difference between the impression one gets reading Baker's original papers and reports on the early work of developing the new plastic design procedures and the version he presents in the book. By this time he is writing with the deceptive familiarity of someone who had helped to create and develop the radically new concepts upon which the new design procedures were based, although an interesting illustration of his awareness of the newness of the concepts is the use of inverted commas around phrases such as 'full plastic moment', 'plastic hinge', 'shape factor', 'failure', 'collapse', 'incremental collapse' and 'shakedown' [e.g. Baker *et al.* 1956:15,34,179]. Nevertheless, for the most part he plays down the originality of the new way of looking at the behaviour of steel-frame structures and the fact that none of the new concepts had even *existed* only 20 years earlier. Baker was, in fact, behaving in a way which is characteristic of the period following a revolution—he was re-writing and re-interpreting history in terms of the new paradigms. Looking at it another way, of course, he was engaged in the process of trying to persuade other engineering scientists and designers to join the new scientific and design communities associated with the plastic behaviour of steel.

11.5 PLASTIC DESIGN PROCEDURES

The shift to plastic design procedures involved a change in every aspect of 'seeing' a real structure under load. A 'plastic' designer came to see, quite literally, a different type of material, type of structure and even type of loading, from his 'elastic' counterpart. The new set of engineering concepts allowed him to think about a structure and its behaviour in a new way. It was this different way of conceptualizing and 'seeing as' which also enabled him to build different mathematical models of the material, structure and load, and thus be able to work quantitatively upon both proposed and existing building structures.

Steel (and most other materials) had previously been treated as perfectly elastic up to a certain limit, beyond which the material's behaviour was of no interest to a designer. The new way of thinking of steel was as an elastic material up to a limit and perfectly plastic thereafter; or rather up to a limit well beyond any likely deformations in a building. This notion of 'perfectly plastic' was itself new, at least as far as structures were concerned. It allowed account to be taken of the (already intuitively well-known) fact that, after yield, a material continues to require energy to continue its deformation even though it will, if unrestrained, deform without limit. In the popular statically determinate structures of the 19th century, such behaviour would

clearly have been catastrophic. In redundant structures, however, such behaviour is tolerable within limits.

With the benefit of hindsight, such a change of attitude appears almost insignificant, especially since the phenomenon was already well-known. The ability of wrought-iron and steel to redistribute stresses by local plastic deformation was known well back into the last century [Unwin 1911/12:12]. Indeed the very ability to shape wrought-iron and steel for use in construction depends on its very plasticity. The reason for the far-reaching consequences lies in the fact that the same phenomenon was regarded from two different points of view—in the one, the plastic region was 'dangerous' and to be avoided, because it was unknown; in the other, it was safe and to be (almost) encouraged, because it was known. What is more, the behaviour in the plastic region turned out to be more highly predictable than the elastic behaviour had been (i.e. the actual stress levels predicted when using elastic design procedures).

With the new model of the material there followed the second new concept, a building block for models of a structure: 'the plastic hinge'. This notion describes the state when the material in the whole of the section of a beam loaded in bending has passed into the plastic region; under the continued application of the load the hinge is able to rotate freely while offering a constant resistance to rotation, the 'full plastic moment'. In a single simply supported beam, such a state means, of course, that it can no longer support the applied load and the structure will have failed. For a more complex structure, however, the possibility of collapse depends not only upon the state of any one member, but also on the state of the structure as a whole - collapse can occur only when there are sufficient hinges to allow it to do so, by becoming a mechanism.

This new way of conceiving of 'failure' and 'collapse' was the third major new concept (paradigm) of plastic design procedures—it represented a fundamental change to the design criteria. Viewing the potential collapse of a structure in this new way had the important advantage that, from the point of view of safety, it became not only desirable to incorporate a certain amount of redundancy into a structure, but also possible to take it into account when designing. A particular advantage of this possibility was that it followed so directly from our intuitive understanding of structural behaviour. So it was that the hundred-year-old habit, of seeing statical determinacy as an ideal to approach, could be broken. It was now possible to tackle redundant structures directly, without the need for unrealistic assumptions being made in order to create a mathematical model which was simple enough to use.

The final stage of the beginning of the 'plastic era' was to consolidate the new approach formally. The new paradigm concepts had arisen out of direct observation of actual structures but they could not be used quantitatively with confidence until the validity of the mathematical modelling to which they seemed to be amenable, was formalized. This required the establishment of an axiomatic system incorporating certain unproved statements about the new concepts (precise *definitions* and *axioms*), and *rules of inference* which, together with the axioms, enable *theorems* to be generated and proved.†

† For a full discussion of the axiomatic systems underpinning various branches of mathematics, see the following—for geometry, see [Hospers 1967:193–198]; for statics and continuum mechanics, see [Noll 1959]; for plastic theory, see [Horne 1949/50].

Plastic design procedures finally reached their maturity when the early theorems of mathematics of plastic theory had been developed to give theorems of direct use to designers. The 'safe theorem' allows a designer to establish with confidence an absolute minimum load which a structure can support by means of a possible distribution of stresses. The 'unsafe' theorem ensures establishment of a maximum load more than which a structure can definitely not support, since a greater load would provide sufficient energy to drive a collapse mechanism [Baker & Heyman 1969:130–136].

A final word in this section is needed to note the different view of the loading of a structure required by plastic design procedures. In elastic methods, the model of the loads on a structure was chosen in such a way as was believed to give rise to maximum stress levels in the structure. The structure was designed so as to keep these maximum levels at or below the working stress of the material, which itself was lower than the yield stress by a factor of safety. One of the findings of the SSRC had been that the usual assumptions about the pattern of loads giving rise to maximum stresses were incorrect. Rather than fully loading each floor of a frame building it was found that other patterns, such as loading alternate floors of alternate bays, could result in higher stresses. The investigation of different patterns such as these, once discovered to be advisable, was easily achieved with elastic design procedures by using the principle of superposition (one of the theorems of elasticity).

The approach of plastic design procedures had to be different because there is no theorem of superposition in plastic theory (such an idea has no meaning). The notion of 'proportional loading' was introduced whereby a chosen pattern of loads, in fixed ratios, is imagined to increase until collapse is reached. In this way, the amount by which the 'working loads' have to be increased to cause collapse can be established, and this factor is a true 'safety' factor, but called 'load factor' to avoid confusion with the 'factor of safety' of elastic procedures. This approach, then, is the inverse of that in elastic procedures which seeks a worst loading pattern which will still give a chosen factor of safety. (This load factor method can also be used in elastic procedures; indeed its use was recommended by the SSRC, but even the idea of 'imagining' a structure being loaded right up to failure was so horrific to some Committee members that 'the same result had to be obtained by subterfuge' [Baker 1935/6:182].)

11.6 THE NEW DESIGN COMMUNITY

By the early 1950s a new 'plastic' design community had formed itself. It talked a new language; it saw structures differently; it had an utterly different approach to design and, not forgetting the past, it had found a new way forward beyond the barrier to further progress which Baker and others had encountered when trying to develop the use of elastic design procedures. The new plastic design procedures were able to give back to designers the confidence to aim to use less steel in their buildings which the SSRC had virtually taken away from them when it exposed the fallacies underlying the elastic design procedures. The power rationally to justify designs had been restored. The ability to describe (define) them had also been improved since, for many structures, the plastic design procedures enabled members to be designed directly, rather than be used only to check an already chosen design, as had been the case with elastic design procedures.

The plastic design revolution exhibits two more phenomena associated with Kuhn's model of progress through revolutions. The first is the manner by which the new community grows. In the absence of a commonly agreed set of paradigms (language, concepts, aims etc.) it is not possible for the transition from the one community to the other to be based upon objective logical argument—the two systems are incompatible. The change has to be based upon subjective appraisal of both sets of paradigms. The first people to change were, then, those who had already been sceptical of the elastic procedures, many of whom had been associated with the work of the SSRC. Under the influence of several particular university departments, a new generation of designers was educated into the new ways, by several of the one-time members of the SSRC.

Many practising designers were easily persuaded of the success of the plastic design procedures by their results, which were to be seen in ever-increasing numbers from the mid-1950's. However, there are even today still many 'elastic' designers who have not changed their allegiance. Examples of written debate between members of the plastic and elastic communities are still to be found occasionally. As is to be expected, the discussions exhibit a failure to communicate at the most fundamental of levels and inevitably end with a polite agreement to disagree (see, for example, [Heyman 1967; Heyman 1973; Beal 1979]). Although all the relevant Codes of Practice for design have now been modified to allow the use of plastic procedures, none has forbidden the use of the older procedures—there is, after all, no evidence that any structures have collapsed as a direct result of using them. In one way, in fact, the elastic methods have received a shot in the arm with the recent development of powerful computers. These are now able to solve the complex and tedious mathematics which had inhibited the pre-war designers, and forced them to make drastic simplifications.

The second post-revolutionary phenomenon to be noted is the progress which has followed the original work on the design of steel frame structures. There has, of course, been considerable further development in that field, but most noticeable has been the transfer of emphasis towards a more general 'limit state' philosophy—a clear example of Kuhnian 'paradigm articulation'.

The idea of the limit state approach had originated in two of the theorems of plastic theory, the safe and the unsafe theorems, also called the 'lower-' and 'upper bound' theorems. Since the mid-1950s the limit state approach has found successful application to materials very different from the ductile steel for which it was first developed. These include masonry, cast iron, timber, reinforced concrete and soils (see, for example, [Heyman 1966; 1967; 1969; 1973; 1982]).

As another strand of his work, and as a further example of the re-writing of history following a revolution, Heyman has even traced the germs of the ideas of plastic design and limit state philosophy (not by name) back to Coulomb's 18th century work on the strength of soils, and work by both Coulomb and Poleni on the stability of masonry structures [Heyman 1972]. Only after the plastic design revolution did the work of these engineers take on what is now seen to be their great importance. Although it is conceivable that this type of development could have occurred independently of the plastic revolution, it is extremely unlikely that it would otherwise have happened so soon and so rapidly. In transcending the barriers to progress set by earlier methods, the plastic design revolution has facilitated such breakthroughs.

Similarly, it is now difficult to overlook even slight hints at the methodology which

has now become accepted, although the following examples are not cited in order to suggest anonymous engineers had foreseen the future at a distance of 800 and 100 years! The first comes from a 12th-century treatise on geometry and explains that the 'duty' (*officium*) of *geometria practica* is 'to give measurements of limits which the work should not surpass' (see §12.3 for full citation). The second was written by an anonymous engineer who was not at all happy with the methods which engineering scientists and theoreticians were trying to get designers to adopt:

> The conclusions of mathematics are often far more exact than practice requires, and that very exactness necessitates the use of excessively complicated formulae. In the present state of knowledge, science would, we submit, be often more usefully employed in determining *relative* than *absolute* results; that is, in *comparing* the efficacy of different modes of constructing, than in determining positive rules for calculating the powers of different machines and stability of particular structures. Where, however, such calculations are required, it would be desirable that attention should be more frequently directed to ascertaining *superior and inferior limits* within which the results must lie. We are justified by experience, and a knowledge of the imperfect state of mechanical science, in concluding, that what it apparently lost in precision by such a course of investigation, would be far more than compensated for by increased security of the accuracy of the conclusions so obtained. [Anon 1854:136]

11.7 CONCLUSION

By way of ending this chapter, in which the work of the Steel Structures Research Committee has featured so prominently, it cannot be emphasized too strongly that Baker, in particular, was singularly aware that the aim of the SSRC was the development of *new design procedures* not new techniques of mathematical analysis. A final citation is given below from a paper he wrote after completion of the Committee's research program just as he was about to embark on his own project to develop plastic design methods. It represents the more-than-usually philosophical thoughts of someone in the very midst of the state of crisis immediately prior to a revolution:

> The designer's task of producing a structure of adequate stength is complex. So complex is it that assumptions must be made, not only to make good deficiencies in the knowledge of conditions but with the avowed object of simplifying the strength-calculations. Whatever advances in knowledge may be made, it is unlikely that this state of affairs will change radically; those of you engaged in design know that involved methods of calculation are usually impracticable. Simplification can, however, be bought at too great a price. There is, in some branches of structural engineering, a tendency for assumptions to be chosen *because* they make some simple calculation possible, even though they are so sweeping that the true behaviour of the structure is disguised. Under these conditions, while it may be possible with the safeguard of large load-factors [or safety factors], to produce stable structures, any evolution of the method of construction is impossible. Although regrettable, this is not surprising. The designer is not averse to adopting improved methods when they are found, but he cannot, except when dealing with a major structure, such as a Sydney Harbour Bridge, afford to devote his time to the search. Other investigators, though giving a great deal of attention to the development of new methods of stress-analysis, have done relatively little to produce new methods of design. Developments in stress-analysis do not influence design directly since they do no more than make easier the determination of the stress distribution in a structure when the sections of the members have been chosen, and it is only in the more important cases

> that the labour of making a tentative design and modifying it by subsequent analysis is offset, economically, by the greater efficiency of the final result. That the production of design-methods has lagged behind stress-analysis is shown by the fact that, apart from the beam resting on simple supports, the only form of structure that can be designed directly by methods common in practice . . . is the simple truss having pin-joints. Even here . . . it is probable that the maximum stress in a strut will not be a close approximation to the value intended by the designer. [Baker 1936/7:297]

Sadly for the engineering designer, the action, in the form of research, which the above passage recommends by implication, is seldom carried out. As will be illustrated in the following chapters, progress towards the improvement of design procedures is often stimulated only by the loss of human life.

12

The Greek and Gothic design revolutions

12.1 HISTORY AND DESIGN PROCEDURES

In discussing the design of buildings before the common use of statics, and yet in the context of a history of structural engineering, one question always raises itself—in what sense were, say, Greek designers designing structurally? Well, if one defines 'designing structurally' as considering the applied loads, resulting stresses and fracture strengths of beams, clearly, they were not. However, they were undoubtedly designing buildings; and these incorporated what we now call load bearing structures. There is also no question that they understood the nature of loads and the capacity of different materials and structures to resist them. Several long-span stone beams (architraves) of U-, inverted T- and L-sections display the Greeks' accurate awareness of bending, and of the relative weakness of stone in tension (Fig. 12.1). One beam, dating from the 4th century BC, also shows an understanding of where the bending moment in a beam is greatest (Fig. 12.2).

Quite how the dimensions of structural parts of buildings were decided, we do not know. It would seem that experiment and precedent are the most likely influences. We must also recall, however, in the case of the designers of Gothic cathedrals, that, an important source was the dimensions and proportions for certain structures contained in the Bible. The Greeks too may have had similar precedents which carried a power of justification much greater than merely what another building designer had done. What does seem to be the case is that, whatever their justification, the dimensions of beams and columns could be incorporated into the design procedures for buildings as proportions or multiples of other dimensions and simultaneously include the appropriate criteria of structural adequacy. As has been mentioned elsewhere in this book, such 'rules', in terms of how they were used by designers, were the direct ancestors of 'grounded rules' and modern design procedures based upon statics.

I would, then, with the qualifications just given, like to answer the earlier question by saying that Greek (and Gothic) designers *were* designing the structures of buildings in as much as they were designing buildings, parts of which had a load-bearing

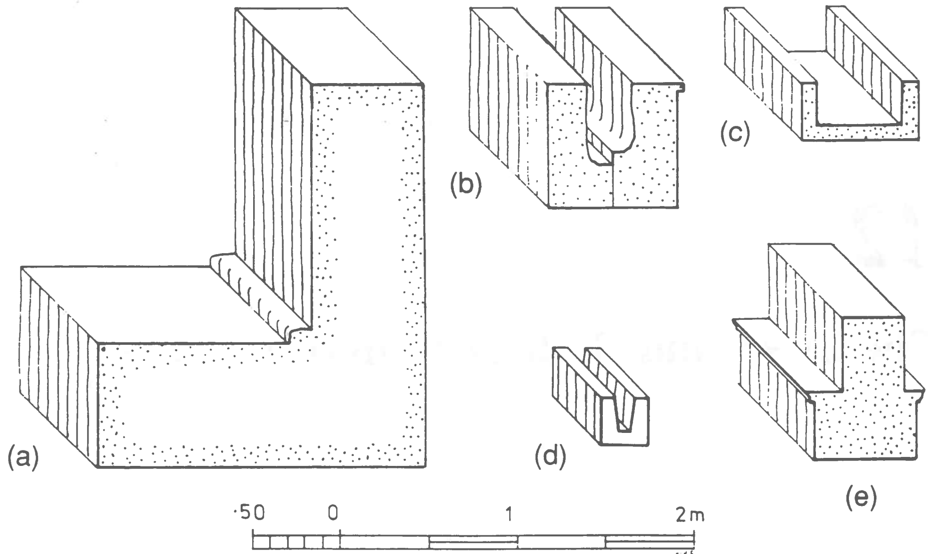


Fig. 12.1—Sections of stone beams found in Greece. (a) architrave, 6th century BC; (b) architraves, c. 500 BC; (c) ceiling beam, c. 420 BC; (d) lintel, c. 630 BC; (e) cross beam, c. 320 BC. [Coulton 1977:145]

function of which the designers were aware, and which worked satisfactorily. It is an advantage of the approach advocated in this book that structural design is treated within its overall context of building design: as and when more and more sophisticated means of describing and justifying a design have been developed, so they have been integrated into the design procedures for the building as a whole. Inevitably with the passage of time, specialist areas within overall building design procedures have developed their own identity and tended to separate off—just 'natural philosophy' before the 18th century has 'given birth' to the specialist subjects of physics, chemistry, biology, biochemistry and so on.

A second question which needs to be considered concerns the suitability of using Kuhn's model of the historical development of science in the area of building design. It has been shown in the previous chapter that the recent radical change of approach

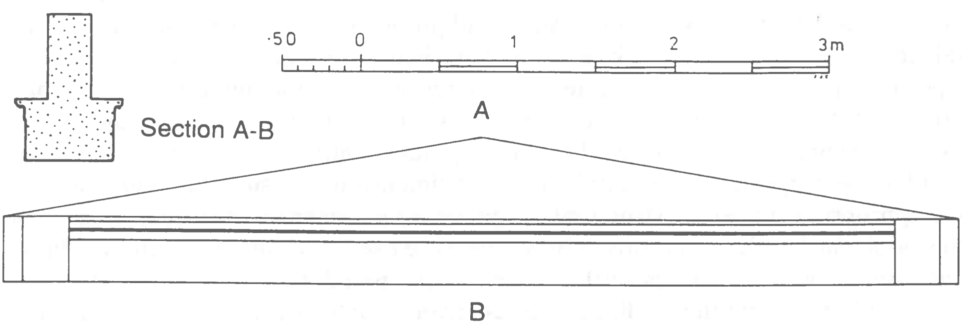


Fig. 12.2—Stone beam supporting a marble coffered ceiling over the porch (pronaos) of a temple at Samothrace (6.15 metre span). [Coulton 1977:146]

to designing steel buildings did display many of the characteristics of a Kuhnian scientific revolution as long as one was careful to focus attention on the engineering designer and the designer's equivalent to the scientist's hypothesis, namely the design procedure. Kuhn's approach to the history of science shares much with Darwin's approach to the origin and history of species. As well as being particular to their respective fields, they also have a generality which transcends the specialist subject. The aspect of Darwin's work which most disturbed his contemporaries was that the changes which species have undergone throughout history were not 'goal directed' (by a greater power) towards the production of man as the pinnacle of development: contemporary cartoons viciously portrayed Gods playing dice to decide the outcome of history. Similarly, Kuhn's critics have had difficulty in accepting that scientific progress might not have been always leading inevitably towards modern Western science.

As was mentioned earlier in this chapter and in Chapter 2, it is possible to take a strict interpretation of structural design when looking at the history of the subject and to scorn virtually anything but the most recent design procedures which closely resemble modern design practice. Alternatively, it would be possible to portray the history of design procedures in an evolutionary (Darwinian) way. To view the development of design procedures in this way would describe them as being 'born', perhaps as chance mutations, developing healthily as long as they could adapt to the surroundings, and dying out when they failed to adapt adequately to a changing environment. But such a model feels rather too fatalistic and impersonal to be applied to so human an activity as designing a building.

The difference between Darwinian evolution and Kuhnian development of science is that man has had a hand in the latter and so human psychology (intention and subjectivity) has needed to be taken into account, as well as the objective nature of the world about us. In this sense, designing buildings is even more of a human activity than science is. Kuhn's model of historical development is particularly appropriate to help interpret the history of design procedures because it deals with change in a field which can be formalized (unlike politics, for instance) while acknowledging the importance of human factors such as subjective knowledge, beliefs and intentions, intellectual and linguistic concepts and human skills. Progress in structural engineering has been as much due to breaking down intellectual barriers as technological ones.

12.2 THE GREEK DESIGN REVOLUTION

As with many fields of human endeavour, the classical world of Greece provides us with some of the earliest evidence of the development of Western culture. In the design of buildings, the 7th century BC saw a rapid transition from small-scale and relatively temporary folk architecture to a monumental architecture (initially comprising mainly temples) which was on a very large scale and had to be long-lasting. This change posed a great many problems for builders at the time who were suddenly asked to cope with many new issues which had previously not arisen.

A painter, sculptor or builder of a small building, had generally been able to conceive the whole finished work, to execute it himself with little help, to develop the

design as it progressed, correct errors at almost any stage, and if necessary, abandon the work and start again. This was not the case for the designer of large-scale buildings who:

> must always start his buildings at the bottom, and cannot modify at all what he has built first in the light of what follows. Mistakes made at the start can therefore not be corrected, and they will also be ruinously expensive, for a monumental building will occupy many men for many years. For these reasons an architect more than any other artist needs a technique of design, a technique which will allow him to visualize the finished building beforehand with sufficient accuracy to ensure that the lower parts of the building will suit the parts which are to be put on top of them, and that the whole buiding is satisfactory in form, function and structure. Design work in a rather different sense is also needed to communicate the architect's intention to the builders . . . The Greeks were not of course born with these techniques ready formed in their minds. [Coulton 1977:51]

> Greek architects were apparently slow [some 200 years] to see the need to define precisely at an early stage parts of a building which would not actually be required until much later. This slowness was probably due in part to the fact that they were breaking new ground. In no earlier architecture known to them did the upper parts of a building make such precise demands upon the design of the lower parts, so that it was not immediately obviously what was required, and how it should be done. [Coulton 1977:64]

Coulton argues that one of the major achievements of the Greeks in the following few centuries was the development of sophisticated design procedures. These design procedures enabled designers to plan their buildings and to control the way they were to look. They enabled designers to communicate the designs to the workforce and to other designers who might, on a large work, succeed the original one. They enabled the designers to present and to justify their designs to clients. Finally, and perhaps most importantly, they allowed controlled experimentation and development of the designs which could then easily be transmitted over large areas of the country. (Whether for this reason or not, Greek architecture developed more in a century than Egyptian architecture had in 3000 years.) In short, it was the development of the design procedures which enabled the Greeks to create, experiment with, modify, develop, communicate and propagate their characteristic methods of building design and architectural style.

These developments by the Greeks constituted a major fundamental revolution in the way in which building design was carried out. There is, of course, no documentary evidence to substantiate all the stages of a Kuhnian revolution, but the results found in the buildings the Greeks produced do provide considerable information about their design procedures and how they developed as well as evidence of distinct design communities. In addition to this main change in design procedures, a number of smaller ones can also be discerned.

The design procedures developed by the Greeks made use of numbers, simple proportions and geometry for both the description and the theoretical justification of designs. Rules based on precedent in nature, especially on the 'design' of the human body, were particularly important. Vitruvius (writing some centuries later, but basing his work not only on current Roman practice but also on ancient Greek architectural treatises now lost to us) justifies the importance of the numbers 2, 3, 4, 6 and 10 in terms of the human body, and of the numbers 6 and 16 (=6+10) in mathematical

terms. The circle and the square had both geometric and anatomical justification. Concerning the use of rules 'founded on the analogy of nature':

> if it is agreed that number was found out from the human finger, and that there is a symmetrical correspondence between the members separately and the entire form of the body, in accordance with a certain part selected as standard, we can have nothing but respect for those who, constructing temples of the immortal gods, have so arranged the members of the works that both the separate parts and the whole design may harmonize in their proportions and symmetry. [Vitruvius 3.1.9]

A typical example of the type of design procedures which the Greek architects developed are those needed to set out the columns for a temple of rectangular plan. One such problem arises when choosing the precise spacing of columns on the stylobate (the platform on which the columns stand). The inter-columnar spacing is of great importance in a Doric temple since it is one of the fundamental dimensions from which most other dimensions are derived. Of the several ways of setting out a certain number of columns on a rectangular grid, only one option results in equal column spacings along both the sides and the ends: other options give unequal spacings (Fig. 12.3). The earliest temples in the late 5th and early 6th century BC adopted the simple regular solution, but sometime during the late 6th century, designs changed to favour placing the columns at the perimeter of the planning grid (Fig. 12.3b). This not only gave greater inter-columnar spacing along the ends but also changed the proportions of the basic rectangle (from 6:16 to 6:16.5 or 6:17, depending on column diameter, in this example). Whether this change was for aesthetic, functional or more philosophical reasons, we do not know. One consequence of the change is that there are now two different inter-columnar distances which can be used to derive other dimensions. It is easy to imagine the lively debates there must have been as to the best way of proceeding, especially when one remembers that number and geometry meant much more to a Greek designer than merely being branches of mathematics—

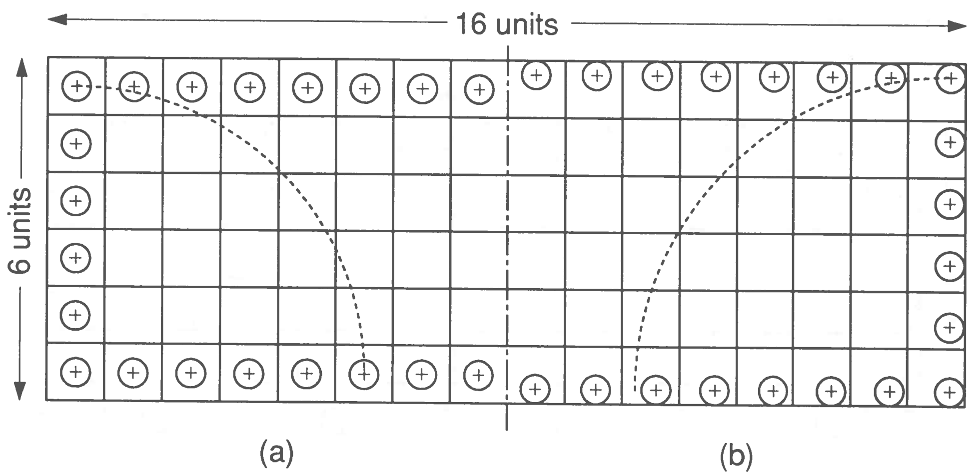


Fig. 12.3—Hypothetical temple plan with 6×16 columns: (a) columns centred in grid squares, giving equal spacing; (b) columns set at the edge of the stylobate (platform) giving greater intercolumniations on the short side (temple front). [Coulton 1977:60]

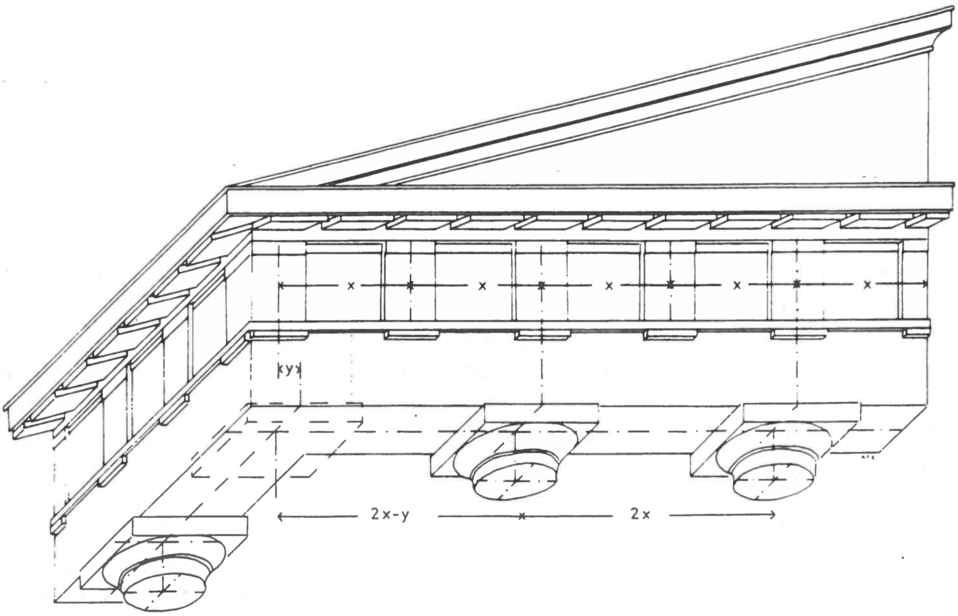


Fig. 12.4—Angle contraction in a Doric entablature [Coulton 1977:61]

they were direct evidence of the harmony which was supposed to be found everywhere in nature and, to some extent, the very use of simple number ratios and geometric figures justified the products which resulted (see §6.2).

The problem of column spacing is made even more complex by the need to reconcile the conflicting requirements of regularity in the column spacing and the proportions of the frieze above the columns (Fig. 12.4). The amount of correction to the regular column spacing which is needed at the corners depends not only on the geometry of the frieze but also on the dimensions of the column, both at the base and

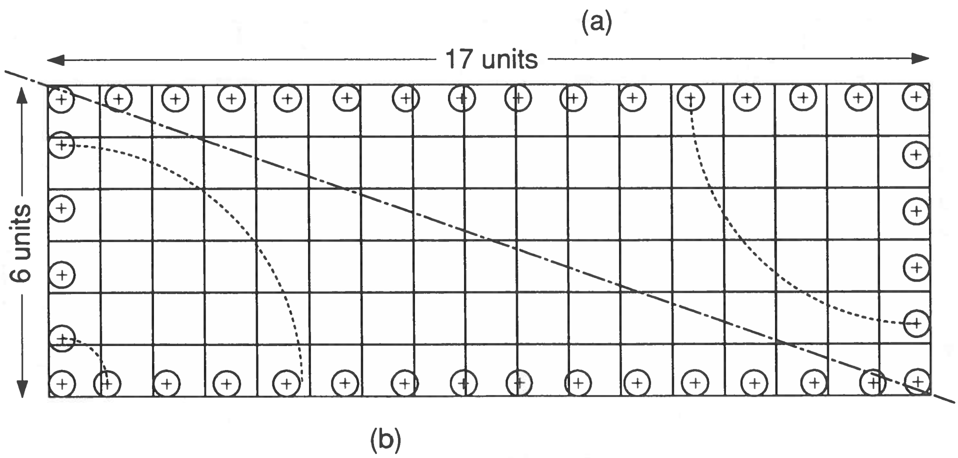


Fig. 12.5—Hypothetical temple plan with 6×16 columns: (a) without angle contraction and with equal column spacing; (b) with angle contraction giving greater intercolumniations on the short side (temple front) [Coulton 1977:62]

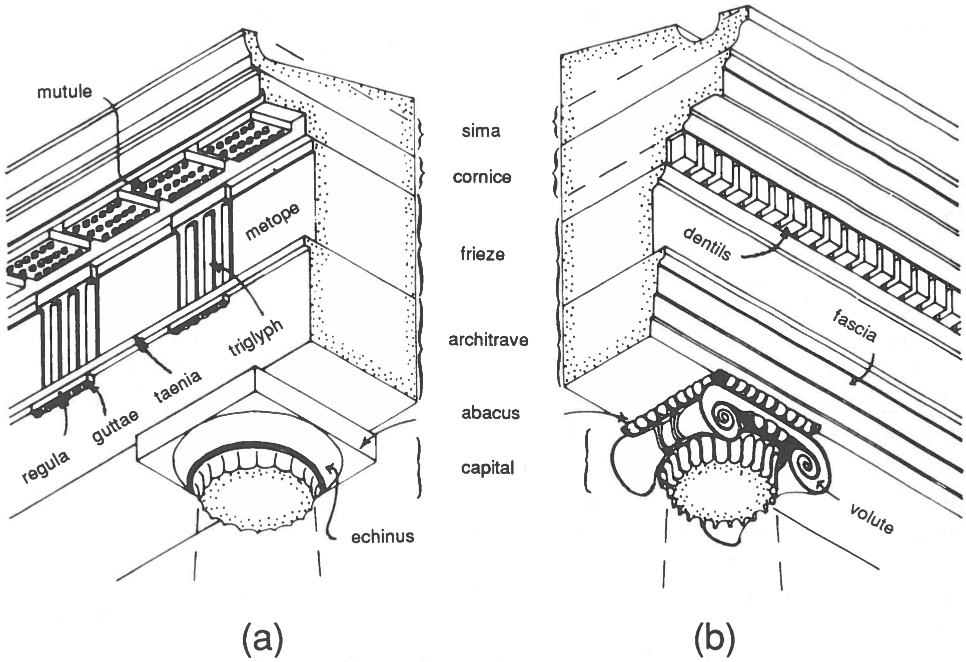


Fig. 12.6—Details of Doric and Ionic entablature

at capital level. Suddenly what seems a simple matter becomes highly intricate and requires the assistance of some mathematical techniques to keep track of the complex inter-relationships (Fig. 12.5). It clearly required very sophisticated design procedures to follow through the design of an entire temple and it took some 200 years of development before satisfactory procedures were achieved [Coulton 1977:59–65].

The Hellenic period (c. 800–323 BC) of Greek architecture was characterized mainly by two co-existing 'design communities', as one might call them—the Doric and the Ionic orders. They were both established around 600 BC, respectively on the Greek mainland, Southern Italy and Sicily, and in Asia Minor and the Mediterranean islands. The differences between them were not only in the appearance of the buildings they designed but also in their design procedures (Fig. 12.6). (In fact, a third order, from Corinth, had existed at about the same time but died out rather quickly; it was reintroduced by the Romans who also developed the Tuscan order: both appeared in Vitruvius).

As with modern design procedures, much that influenced the design of a Greek temple was part of common knowledge and was not written down in detail. Certain fundamental decisions which affect both the elevation and the plan of a temple are expressed in terms of its 'style'. The front of a Doric temple, for instance, is the short side of a rectangle of side-ratio about 2:1 (Vitruvius says exactly 2:1 but others evidently thought otherwise). According to the chosen style it may have either four (*tetrastyle*) or six (*hexastyle*) columns. The intercolumniation also depends on the style—it may be *systyle* or *diastyle* having, respectively, two or three metopes between

columns. In each case the architrave between the centre pair of columns contains an additional metope. The styles for Ionic temples are more varied and open to greater flexibility. The front of an Ionic temple is the short side of a rectangle. According to the chosen style it may have either four, six, eight or, occasionally, ten columns—respectively *tetra-*, *hexa-*, *octa-* and *decastyle*. The long side generally has one less than twice this number of columns. The intercolumniation depends on the style of the temple and is a function of the diameter of the column at its base (*D*). The distance between column centres for the various styles is as follows: *pycnostyle* 2.5*D*, *systyle* 3*D*, *eustyle* 3.25*D* (4*D* between centre pair of columns), *diastyle* 4*D* and *araeostyle* 5*D*.

The Doric design procedures, as described by Vitruvius, were elegantly simple in conception. They required the selection of a single fundamental 'module' equal to one half of the diameter of a column, 'and this module once fixed, all the parts of the work are adjusted by means of calculations based upon it' [Vitruvius 4.3.3] (see Fig. 12.7). Incidentally, Vitruvius used the Greek word *embatis* for 'module' as there was no suitable Latin word. According to the particular set of calculations used, so a design procedure for the various dimensions of an entire building could be built up with the designer able to select particular multiples and ratios to suit the purposes—the designer was effectively creating a mathematical model of the building.

It is, in fact, likely that the earliest Doric design procedures had not been based upon a single module, rather they involved also some sequential rules in the manner of the Ionic procedures (see below), but simpler. The state of the art of designing buildings, as presented by Vitruvius, was the result of several centuries of development towards ever simpler design procedures, refining them as the results of their repeated application were observed:

> There seems . . . to be an increasing concentration on the small elements of a building as the basis of the proportional system. The width and length of the stylobate seem initially to have been related to each other directly, and Pliny records that in early temples the column height was made one third of the width . . . Later the stylobate width and length were derived from the intercolumniation, and so too, in some cases, was the column height. The final stage in this development is reached with Vitruvius' rules for Doric, according to which a module was first derived and all the elements of the order, down to the smallest detail, were simple multiples or fractions of the module. [Coulton 1977:65]
> 1977:65]

We do have evidence that the Greeks were using similar design procedures based upon a single basic dimension in the 3rd century BC in another important branch of engineering—the design of artillery. The diameter of the spring for use in a large catapult was expressed as a certain proportion of the cube root of the mass of the projectile to be fired. When in use, the range of the catapult, too, could be calculated from the projectile mass [Philon; see Marsden 1971:107ff].

Ionic design procedures, on the other hand, were originally, and remained, more complex than the Doric ones:

> [They] do not relate each element to a single common module, but form a sort of chain, so that each element is derived successively from a preceding one, usually the immediately preceding one. The ratios between successive parts are also more complex than in the modular system, and the ratios between widely separated parts may be very hard to calculate. Because of this structure, such a system gives more scope for experimentation and variation [see Fig. 12.8]. [Coulton 1977:66]

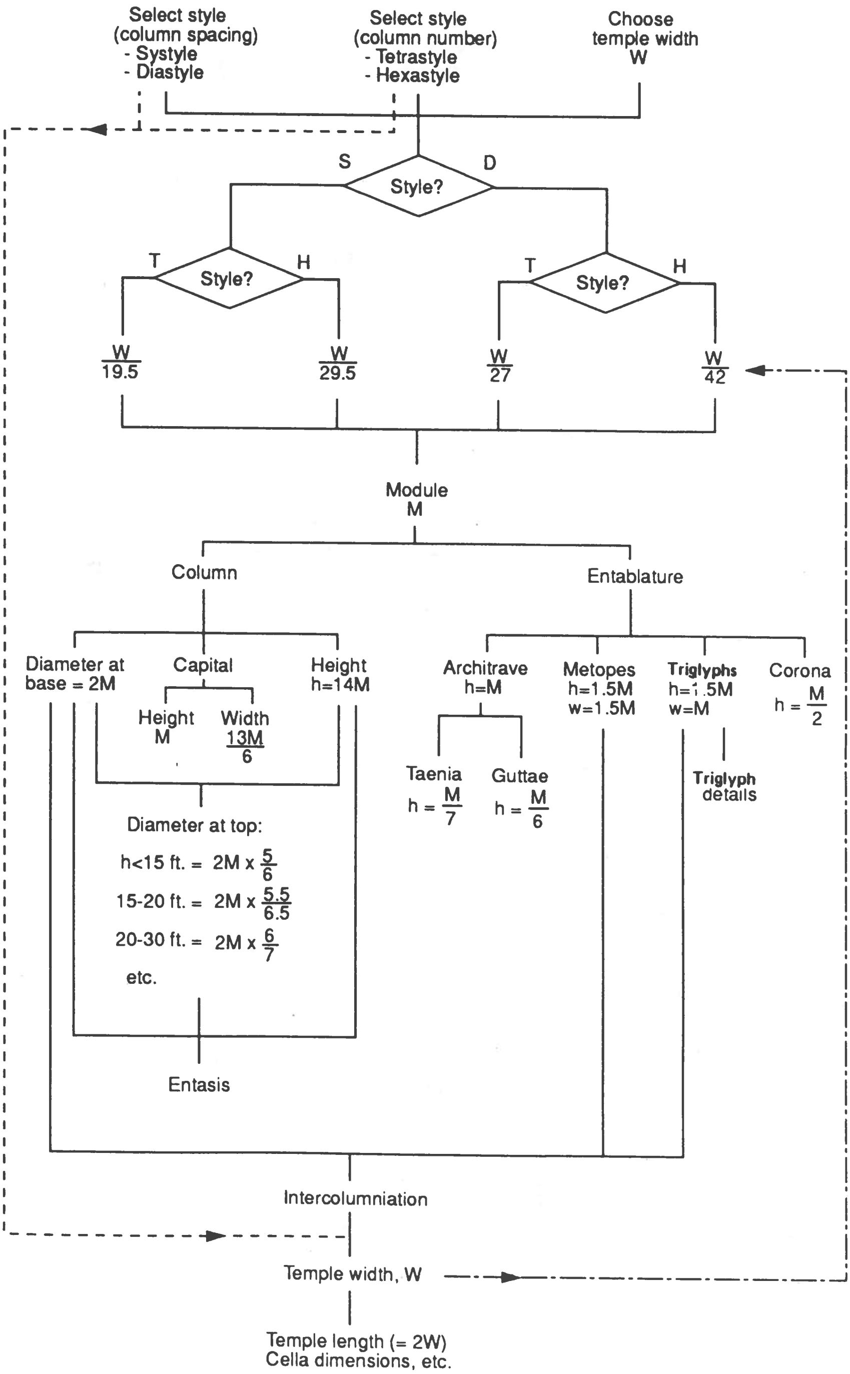


Fig. 12.7—Design procedure for the Doric order, according to Vitruvius (omitting the finer details)

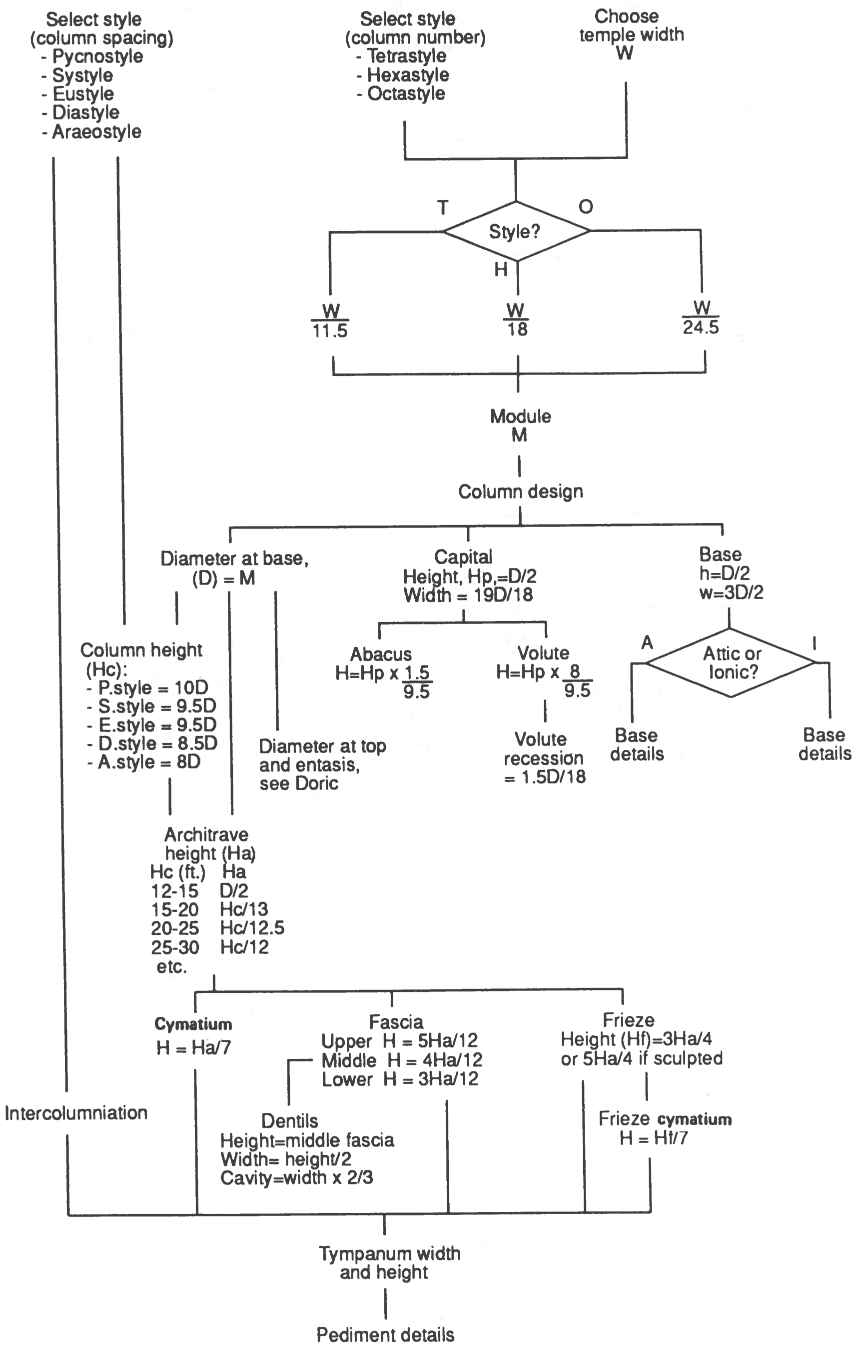


Fig. 12.8—Design procedure for the Ionic order, according to Vitruvius (omitting the finer details)

Although both of the two design communities co-existed over several centuries, by the time Vitruvius was writing (c.25 BC), the Doric procedures were much less favoured because of their inflexibility and their inadequacy in dealing with the problems of column spacing and frieze design at the corners [Coulton 1977:66]. Vitruvius draws attention to these 'faults' and appears almost reluctant to set down the Doric procedures for his readers [Vitruvius 4.3.2].† The Romans soon discarded the Doric order and, in addition to developing the Ionic order, resurrected a design which the Greeks had abandoned some centuries earlier - the Corinthian order. Finally in the Hellenic period, it is interesting to note some clear evidence about design changes in the cases of two particular design details—the bases of Ionic columns and Doric capitals. Over a period of some 200 years each went through several distinct periods of little change throughout the whole country, which were separated by very short periods of sudden and rapid change [Coulton 1977:99-107]. While we do not know the reasons for these changes, the pattern does follow the non-continuous pattern of change which characterizes Kuhnian development.

Following the Hellenic period, the Hellenistic period (323–31 BC) brought some minor changes to existing design procedures, such as a new system of writing numbers which facilitated the calculations involved in design. But, at a deeper level, the era brought a new emphasis into architecture which the old design procedures were unable to cope with satisfactorily. There was a move away from the design of single familiar buildings towards a much wider range of new and complex building types. The emphasis also shifted towards the designing of groups of different buildings in relation to one another [Coulton 1977:68].

It is likely that such complex and unique problems stimulated the need for architectural drawings which had not been required for the design procedures of the earlier age, when the problems were less varied and simpler. Certainly, there is no evidence for drawings during the Hellenic period, whereas by the end of the Hellenistic period, when Vitruvius was writing, the plan, elevation and perspective drawing were a normal part of an architect's work [Vitruvius 1.1.4, 1.2.2]. It would be interesting to establish whether this design revolution, the change to designing geometrically rather than numerically, was discernible in the resulting buildings. Unfortunately, the Greek Empire was already near its end and few buildings from this period remain.

12.3 THE GOTHIC DESIGN REVOLUTION

The cathedrals of the so-called 'Gothic' period of architecture hold a unique position in the history of building construction in that their supreme greatness is acknowledged by 20th-century engineers, architects and the general public alike. The beginning of the Gothic period can be pinpointed quite accurately as about 1134: in or around that year new building commenced at the cathedrals of St Denis and Chartres. Within a few years several others were begun, all in the region around Paris called the Ile de France and by the year 1300, some 60 Gothic cathedrals in France and 40 in England, were complete or under construction. The style continued to spread throughout

† Vitruvius devotes almost the whole of his Book 3 to the Ionic order but only a chapter each in Book 4 to the Corinthian, Doric and Tuscan orders (in that sequence).

Germany, Italy, Spain and central Europe until the late 16th century. The question as to how the cathedrals were built is largely known [e.g. Fitchen 1961, 1986; James, J. 1982] and is even the subject of several excellent children's books in both English and French [e.g. Macaulay 1974]. The question as to how they were designed, however, has been a matter of great contention.

The skill with which the vaults and flying buttresses of a Gothic cathedral make impressively economical use of material has led some writers to conclude implicitly, and even explicitly, that the cathedral designers must have had a knowledge of statics [e.g. Hertwig 1934; Frankl 1960; see §2.4].† Other historians of engineering acknowledge that statics, as we now use it, was not available and yet virtually dismiss the design of cathedrals as *merely* empirical, and refer vaguely to a supposedly large number of collapses which 'prove' that designs were based upon *trial and error.* This seems to be a rather harsh judgement of the skills of the designers and builders of such remarkable structures as Chartres and Rheims cathedrals. In fact, the number of failures has not been so very great and nearly all of them have been due to substantial movements of the foundations. Above ground, where simple statics is at work, so to speak, Heyman's version of the 'safe', or 'lower-bound' theorem (from modern limit-state design methods) applies:

> If, on striking the centering for a flying buttress, that buttress stands for 5 minutes, then it will stand for 500 years. [Heyman 1966:254]

We do now know quite a lot about the design of Gothic cathedrals. We know the names of many hundreds of the individuals such as Henry Yeveley, the master mason for Westminster Abbey, who were involved with the design and building of the cathedrals. We also have several documents relating directly to the design of cathedrals. There are several sketchbooks of details and plans of cathedrals, the most famous of which is by Villard de Honnecourt. There are several surviving more substantial drawings including complete plans and elevations of Strasbourg cathedral (c.1270). We also have both direct and indirect evidence of how cathedrals were designed (including the buildings themselves, of course).‡

In looking for the structural content of Gothic cathedral design, we need to consider the 'design procedures' which the designers might have used. These, it will be recalled, provide the 'description' and a 'justification' for a proposed design. For the description, geometry was used in the form of drawings, wooden templates and memorized sequences of geometric constructions and operations. For the justification, precedent was probably the dominant means, but here the story becomes more complicated and a certain amount of background and conjecture is needed.

We have no explicit design procedures dating from the high Gothic period. This is hardly surprising considering the secrecy surrounding the skills of the trades such as master mason—it was forbidden to divulge any information outside the masons'

† I use 'designer' to refer to the hypothetical individual who designed a cathedral, mindful of the fact that the full job was probably carried out by many individuals belonging to several professions. While a master mason would be the principal person, it is likely that others, including the client, his adviser or representative, were also involved. I deliberately avoid the word 'architect' which nowadays has such a narrow meaning and misleading connotations.

‡ The reader is referred to the 30 or so works by Ackerman, Bucher, Fitchen, Frankl, Gimpel, Harvey, James, Kubler, Shelby and von Simson, given in the bibliography.

lodge, either to other masons or to non-masons [Frankl 1960]. In Villard's sketch-book it is nevertheless possible to see how some of the plans and shapes have been constructed. The two basic techniques are the use of the circle, for instance to generate a variety of Gothic arches (Fig. 13.1) and the square. By rotating the square, a series is generated of ever-increasing (or decreasing) lengths, each with the same ratio to the next ($\sqrt{2}$). This technique was obviously used to generate the plan of a tower of Laon cathedral (Fig. 12.9). From later in the Gothic period there survive a number of lodge books which contain specific instructions on how to generate the shape of, for instance, a pinnacle by the use of the rotating square (Roriczer, Schmuttermayer, Lechler; see Figs. 12.10 and 12.11). Finally, and from the structural engineer's point of view, perhaps of most interest, is a description from the mid-16th century of how

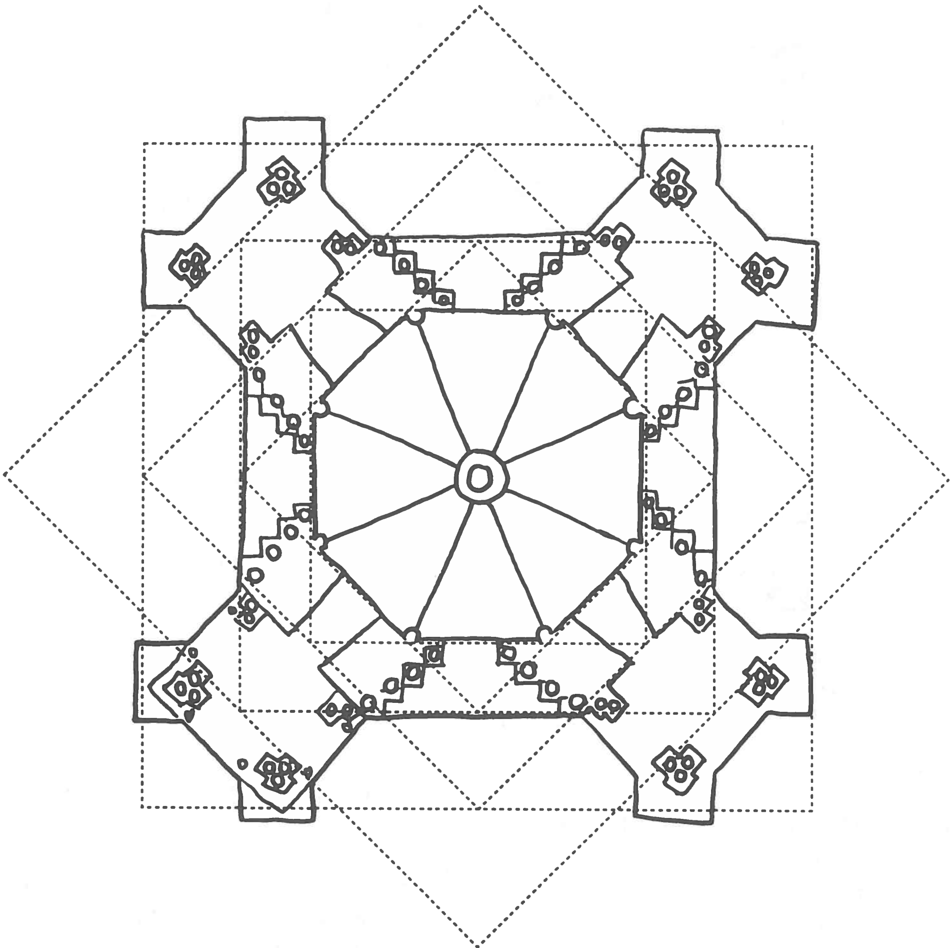


Fig. 12.9—Rotation of the square—one of the fundamental Gothic design procedures, based on a method given by Vitruvius. The plan of the tower of Laon cathedral, completed around 1225, appears in Villard de Honnecourt's contemporary sketchbook. It is shown superimposed on a grid of diminishing and rotated squares which clearly indicate their use in developing the design

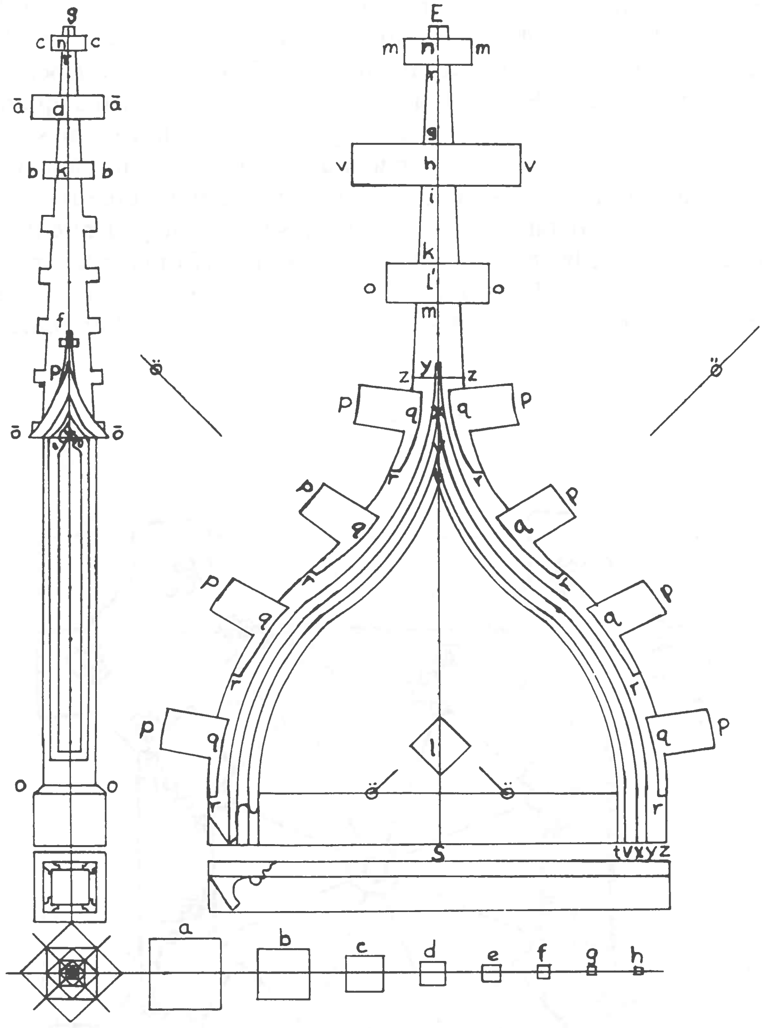


Fig. 12.10—Late Gothic design procedures for a pinnacle (left) and gablet. The author describes every step required to complete the designs. Note the use of the rotated square to generate a set of eight 'preferred dimensions', a–h. All the dimensions in the pinnacle and gablet are multiples of these. [Schmuttermayer 1486; Shelby 1977]

to size the ribs of roof vaults by the use of several geometric design procedures (Gil de Hontañon; see Fig. 13.2–13.4).

Although we may never know how the people at the time perceived what was happening, nor why it happened, there is absolutely no doubt that something extraordinary occurred in the art of building design and construction in the 12th century. The nature of this 'Gothic design revolution' is apparent from the buildings themselves. After several hundred years of gradual development since the end of the Roman empire, the designs for large churches took a sudden and sharp change of direction. Round arches were replaced by pointed arches, long barrel vaults were replaced by several discrete structural 'bays' formed by quadripartite vaults (short intersecting

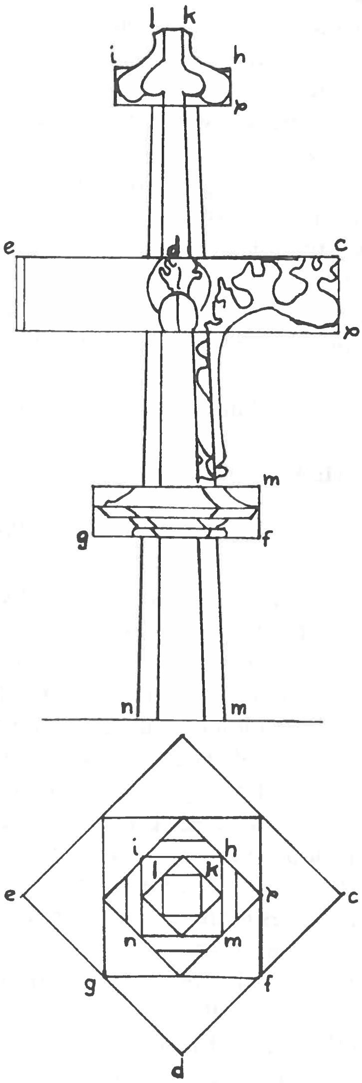


Fig. 12.11—Another design procedure for the finial on top of a gablet similar to the one shown in Fig. 12.10. [Roriczer 1486; Shelby 1977]

pointed vaults), the groin vault was replaced by the ribbed vault, columns were reduced in thickness to an absolute minimum, the maximum area of window in a wall increased from perhaps 30% to around 80%, the horizontal thrusts of the vaults (and wind loads on the roof) were carried out over covered aisles by means of highly efficient flying buttresses. The world would have to wait for 700 years for a similar period of dramatic structural development in early Victorian England.

It is usual to look into the history of technology for the reasons behind such changes and there had, indeed, been several recent and significant developments in

the art of construction, such as the development of more effective lifting devices, scaffolding and piling machinery, for example, as well as improvements in the accuracy of measurement and setting out [Harvey 1972:96–109]. However, as with most technological change, such development would mainly have been the response to a change in the types of buildings which people wanted to build—one cannot explain the developments in railways bridges in the 1840s and 1850s as the result of improved ways of making large castings or of new ways of making wrought iron. Technological developments might enable radically new buildings to be built, but do not directly influence the will to build. The reasons why people should suddenly want to build a new kind of cathedral are no doubt many and touch on issues outside the realm of the present study. There remains, however, that crucial area which lies between the will to build and the ability to carry out the construction—namely, the matter of *believing* that such a radically new building could be built at all.

There is considerable evidence that during the Gothic period there was a steady improvement in the various elements of the cathedral—more slender columns and flying buttresses and so on. However, these changes are relatively small compared to the very sudden developments of the mid-12th century. How did people come to believe that it would be possible to build cathedrals taller, wider, longer and more daring than ever before?

This question is relatively easy to answer for the equally dramatic changes in structural engineering which occurred during the early 19th century. This period saw the gathering of an enormous amount of data derived from tests on materials and entire structures, coupled with developments in the engineering science of strength of materials and the mathematical notion of statical equilibrium. This all led to a much deeper understanding of the world of existing structures and a corresponding degree of confidence to predict the (relatively) unknown or untried. In other words, engineers were able to develop new design procedures which had a very much more powerful ability to justify proposed designs, based both on precedent (test results) and explanatory theories. It is not unreasonable to suppose that there might have been a similar set of circumstances in the 12th century and there is a certain amount of evidence to support such a hypothesis.

At present we still have very few details of how the designers might have tried out their bold designs. We know that small-scale models of wood were made for many cathedrals including Milan [Frankl 1960] and that designers used models as a way of trying out the effectiveness of new design ideas using what has been christened 'micro-architecture' [Bucher 1976]. Such models would have helped to test certain spatial and visual aspects of the design procedures, but would not have helped to justify the safety and stability of the proposed buildings. In this area, the evidence is rarer. In 1514 a guild ordinance in the city of Regensburg required that stonemasons should make models of clay when necessary, though their purpose is not elucidated. There is, however, the interesting case of San Petronio in Bologna.

San Petronio was being designed just after the enormous cathedral at Milan had been begun in 1386. The Bolognese wished to outdo their northern neighbours and conceived a building which would have been the largest cathedral in Europe at the time, had it been completed. They clearly realized the enormousness and boldness of their task for they sent the designer, Antonio di Vincenzo, to both Florence and Milan to look at both cathedrals, or rather part-cathedrals. In Florence he saw a

complete building lacking the all-important dome which Brunelleschi only began to build some 30 years later, while at Milan, apart from the large wooden model, he would have seen only the layout of the building with walls and columns no more than 10 or 15 metres high.

Armed with the experience of his visit, Vincenzo had a substantial model of Bologna's proposed cathedral built in the year 1390. It was made of brick and plaster and was nearly 19 metres long and about 6 metres high (approximately 1/8 full size). It is extremely unlikely that such an enormous model, which people could walk inside, was built only for visual and architectural reasons; and if not, then perhaps it was built as a test with structural significance. Whatever the reason behind the model, its very construction out of materials similar to the masonry with which the cathedral would be built would have constituted a reliable test of the stability of the proposed structure, since the stability of masonry (compression) structures is dependent only on the relative proportions of particular dimensions, *not* on the scale† (this is not the case for structures such as a timber bridge which rely upon significant tension and bending within the materials). Mediaeval designers would not have been able to explain the reliability or validity of such model tests in the way we can nowadays, although their geometric similarity to the real thing might well have been justification enough, as we shall soon see. If the models were well made, the use of such tests would have been extremely reliable in design procedures as a means of justifying proposed structural designs and if such tests were widespread, they would help to explain the geat successes which Gothic designers achieved.

In addition to the use of models and precedent there remains another possible way in which designs for proposed cathedrals could be justified. It will be recalled that two 'sciences' (i.e. bodies of knowledge) served Greek (and Renaissance) philosophers in the way we are now served by the natural sciences, by being able to provide explanations of why the world is the way it is. These sciences were geometry and harmonics and both had featured prominently in Vitruvius' book on building design and construction dating from about 25 BC, of which several manuscript copies and part copies survived into the Middle Ages and were reasonably well known. Although Classical Vitruvian architecture had died out with the fall of the Roman empire, remnants from both Greek and Roman philosophical beliefs about harmony and geometry, and much of the Romans' knowledge of building design and construction survived throughout the Romanesque period of architecture [Conant 1968].

There was, however, another important route by which Greek ideas were incorporated into mediaeval life. This was through the writings of the philosophers and hermeneuts St Augustine and Boethius (5th and 6th centuries). It will be recalled from §6.2 how they fused together much of the Greek philosophical heritage, especially harmonics and the work of the Pythagoreans, with that of Christianity and thus demonstrated how the Bible, suitably interpreted, was the source of virtually all important knowledge, including building. The Bible even provided details of certain buildings such as the Ark and Solomon's Temple. Above all, Augustine and Boethius emphasized the central role played by geometry in all attempts to understand the world. These would have been the beliefs which were current amongst philosophers around the year 1100, in particular in the area near Paris:

† This fact has also led to the idea of a 'geometrical factor of safety' which is important to the stability of a masonry structure and can be established using a model of any scale; see [Heyman 1982].

> Augustinian aesthetics were never forgotten during the Middle Ages. In the twelfth century, however, they gain an unprecedented importance in the Ile de France, under the influence of two movements, the first intellectual and speculative, the second spiritual and ascetical. The first centers in the group of Platonists assembled at the school of Chartres, the second in the monastic reform emanating from Citeaux and embodied by Bernard of Clairvaux . . . The aspects of the theology and cosmology of Chartres that interest us most in our present context are, first the emphasis on mathematics, particularly geometry, and second the aesthetic consequences of this thought . . . The masters of Chartres, like the Platonists and Pythagoreans of all ages, were obsessed with mathematics; it was considered the link between God and the world. The most influential exponent of the system, Thierry of Chartres, hoped to find, with the help of geometry and arithmetic, the divine artist in his creation; he went further and sought to explain the mystery of the Trinity by geometrical demonstration . . . It has been said, that, under Thierry's influence, the school of Chartres attempted to transform theology into geometry. [von Simson 1952:11]

In the midst of this religious movement and inspired by it, there occurred a further significant event: Adelard of Bath, who was a student of Thierry at Chartres, translated Euclid's *Elements of Geometry* from Arabic into Latin. Having survived from Greek times only in Arabic translation, he effectively introduced it for the first time into mediaeval Europe (see [Pacey 1974:74ff]). Adelard was a student of Thierry's at Chartres and had visited Gundissalinus in Segovia which had recently been recaptured from the Moors. His translation appeared in Chartres around 1120 (some say a few years later), just 14 years before the cathedral was started.

Geometry had, of course, survived as a practical art throughout the Middle Ages but the appearance of Euclid did improve the level of geometrical knowledge which could be learnt. Improved geometry facilitated the more accurate 'description' of proposed building designs and was of great practical use in the construction process, in setting out the building and enabling the accuracy of the finished parts and their relative disposition to be checked to within better tolerances. Such an improvement alone would have enabled builders to contemplate larger and taller buildings.

However, it was in the capacity to provide 'justification' of designs that geometry probably had a more profound effect. Euclid introduced a crucial new ingredient—the notion of the geometrical proof. This provided the perfect tool for the philosophers at Chartres logically to argue their views and to justify decisions made in a wide variety of contexts including, perhaps, the design of buildings. Just as occurred after the invention of calculus some 600 years later, philosophers put the new theoretical tool to use in every conceivable way and created, quite literally, a new type of geometry—'*geometria theorica*' [Shelby 1972].

The distinction between '*geometria theorica et practica*' was first made by Hugh of Saint Victor sometime between 1125 and 1141 and in doing so he was looking back to the philosophies of Plato and Aristotle to distinguish the practical skills from the theoretical (contemplative) skills which had been made possible by the appearance of Euclid. Hugh put the theoretical tool to good use, for instance, in helping to explain and justify information given in the Scriptures; he calculated that the reported size of the Ark (40,000 inches) would indeed have been large enough to accommodate all the animals of the world and their food (see [Victor 1979:3,32]). A pupil of Hugh's, Richard of St Victor, interpreted many more exemplars from the bible and even argued that the Celestial Temple in Ezekiel looked like a late

Romanesque cathedral. There soon followed a number of geometry textbooks, some purely practical, but others, such as one written around 1140 by Domenicus Gundissalinus, Archdeacon of Segovia, friend of Thierry of Chartres and likely host to Adelard when he was studying Euclid in Arabic, also dealt with theoretical matters. In it he distinguished the two geometries as follows:

> There are two parts of geometry, the theoretical and the practical. The theoretical is that which contemplates proportions, quantities and their measures by the speculation of the mind alone. The practical is when we measure the unknown quantity of some thing by the experience of the senses. (Gundissalinus c.1140, cited in [Victor 1979:22])

Another 12th-century treatise gave a more detailed distinction which clearly distinguished not only the theoretical and practical aspects of geometry but also their respective 'purposes' and 'duties' (Fig. 12.12).

	Geometria theorica	*Geometria practica*
finis (purpose)	to teach something	to do something
officium (duty)	to give reasons and dispel doubt	to give measurements or limits which the work should not surpass

Fig. 12.12—The several functions of mediaeval geometry according to a 12th-century treatise [Victor 1979:9]

The prologue to yet another 12th-century geometry treatise:

> almost makes the practical side of geometry seem subservient or secondary to the theoretical . . . The use of theoretical methods in practical geometry seems to have increased between the twelfth and the fourteenth centuries. At first their role was ancillary to the purposes of practical geometry. Once proofs had found a place in practical geometry, their role increased and changed. Theoretical proof became the goal even of practical geometry. [Victor 1979:45,53]

In discussing 12th-century geometry books, one can see a clear resemblance to much of the discussion in Chapter 1 concerning the theory/practice demarcation which has affected statics during the last two centuries. There is also a striking similarity in the functions of 'theory' in 12th-century geometry and in modern statics and elasticity. Both are used in 'teaching' and in 'doing' (designing and setting out); both are used to give 'reasons'; both are used to give 'measurements'; and, appearing to anticipating modern limit state design by some 800 years, 12th-century geometry also set 'limits which the work should not surpass'. It is also interesting to note that, following the philosophical separation of theory and practice in geometry, it became a common theme in the many of the treatises on practical geometry to discuss the 'interdependence of theory and practice' [Beaujouan 1975; Victor 1979:9] —an early anticipation of the James Forrest lectures on the 'Interdependence of Abstract Science and Engineering', inaugurated in 1893 at the Institution of Civil Engineers (see Chapter 1).

Nowadays we have generally come to accept that geometry is only a branch of

mathematics, and does not carry with it the power of explanation concerning nature that was so widely attributed to it from Greek times until the 18th century. Its relevance to the real world is found in the interpretation given to the axioms and definitions upon which it is based. The act of interpretation is one of the metaphysical aspects of the functions of a scientific theory (see Appendix 2). As we saw in §6.3, the same can hardly yet be said of statics: the phrase 'the laws of statics' is still in use and many students are still asked to perform experiments which they believe are testing these 'laws'.

As with the Greek design revolution, we have no detailed evidence of earlier mediaeval paradigms, anomalies and the onset of a state of crisis which would provide direct evidence for a classic Kuhnian revolution. However, it is highly likely that no-one has yet looked; it may be that there is much more to find. We do have considerable evidence of the radically new way of looking at, and interpreting the world which arose out of the conjunction of Augustine's ideas, a particular school of philosophy in Chartres and Euclid's notion of the geometric proof. Simultaneously, not only was a new type of architecture born, but also a new world view. In the area of geometry alone, there had been a radical change to the way in which it was used and the significance of the results which could be generated by means of its use, both theoretically and practically. Somehow, this revolution in ideas gave the cathedral designers the confidence to attempt, and to achieve, buildings of unsurpassed excellence. I have already suggested a parallel with later developments in structural design procedures when the use of statics seems also to have given designers greater confidence.

A further interesting consequence of the Gothic revolution was a renewed interest in Vitruvius—quickly a large number of hand-written copies were made, of which no fewer than fifty-five survive to the present day, including twelve from the 12th century alone. The remainder date from the next two centuries up to 1486 when the first printed edition of Vitruvius appeared, by which time Alberti had written his own updated version, *De re aedificatoria*, published posthumously in 1485. It is likely that Villard de Honnecourt already knew of Vitruvius when preparing his lodge book (1215–33) and many other stone masons' lodges also seem to have incorporated Vitruvian ideas [Frankl 1960:86ff]. In fact, the most well-known of the Gothic design procedures, the rotation of the square (Fig. 12.9), is described in Vitruvius who attributes it to Plato. Curiously, though, it appears in the introduction to the chapter on astronomy and the method is illustrated by the hypothetical problem of wanting to double the area of a field 10 feet square—perhaps an early example of textbooks using unrealistic examples!

Towards the end of the Gothic era, we do see some evidence of different design communities having developed in different parts of Europe. In 1391, the building of Milan cathedral was halted and the first of a series of 'expertises' was begun. Their brief was to establish whether the proposed design procedure would be satisfactory. A number of experts from different masons' lodges in France and Germany were summoned and offered their views on the suitability of the proposed design procedure and argued for different ones to be adopted. Some of the changes proposed made very little difference to the height of the building (300 mm), and the reason for recommending them would seem to be more to do with the geometric logic upon which they were based—the square, equilateral triangle, Pythagorean triangle, all of which are discussed in Vitruvius. There seem to have been different opinions on the

power of different design procedures adequately to justify the proposed designs [Frankl 1945; Ackerman 1949; Frankl 1960; Heyman 1967/68]. These events happened over 200 years after the birth of Gothic and it is likely that the rigour of the arguments developed in and around Chartres had long since decayed and the power of the arguments to provide adequate justification of design proposals had evidently diminished to the point of crisis (see, also, p. 207).

13

Some other design revolutions

One aim of this book is to advocate a new approach to the history of structural engineering—one which tries to focus on the work of the structural designer. This approach would complement the better-documented histories of the development of engineering science, studies of individual structures and histories of particular structural types.

The example of the plastic design revolution served to illustrate how the way a certain type of structure is designed can change radically according to how the designer thinks about the problem. This modern example suggested taking a similar approach to interpreting the development of design procedures in classical Greece and mediaeval Europe, two periods which saw particularly dramatic developments in building design, respectively about two and one thousand years ago. Before proceeding to make some general observations about design revolutions, it will be useful to look at a few stages in the histories of design procedures for several common types of structure, the beam, the arch, the truss and the suspension bridge. Since each of these subjects deserves a book of its own, the treatment here will serve only as a brief outline and to illustrate the general approach by mentioning a few major changes and design revolutions.

13.1 BEAM DESIGN

The problem of designing a beam only really became a problem in the early 19th century. Before that time beams were made of either wood or stone. We saw in the previous chapter that the Greeks were masters of the art of designing stone beams. They were already aware that, for the longest spans (or lightest beams) it was beneficial to remove material from the upper part of the section (Fig. 12.1) and at least one engineer was also aware of the benefit of varying the section of the beam to give maximum depth at the mid-span (Fig. 12.2). But we should not be too surprised by these facts: in his discussion of bending some 2000 years later, Galileo considered this knowledge to be possessed intuitively by most people, and he refers to the fact that

Aristotle had written about breaking a stick at its centre by pulling it against one's knee and was aware that it was easiest if one placed the hands as far apart as possible. Beams made of stone or wood shaped to give maximum strength would only be required very rarely. A good designer would seek to avoid excessive spans, both to minimize deflexions and to avoid the need to spend extra time and money reducing the dimensions of a piece of materials which occurs naturally in sizes larger than needed. It is nevertheless tantalizing to imagine how the several Greek engineers arrived at the efficient sections they used. The experiments would have been very expensive, even by the standards of a 20th-century Government-funded research programme.

For day-to-day use of both timber and stone beams, it would have required relatively little time, even starting from scratch, to collect empirical data relating the strength, breadth, depth and length of beams made of different types of stone and wood. Within the working environment of a particular trade (e.g. English church carpentry), the range of both the available and the required sizes of timber, would have been few enough to remember. By the 18th century such tables were being published; they were extensive, reliable, and in common use.

The first development beyond tables listing every conceivable cross-section and length of beam, was to express the data in the form of 'grounded rules'—formulae which reflected the work of experimenters such as Musschenbroek, but which were used only to establish *relative* strengths of beams [Sutherland 1984]. In other words they incorporated the variation in the strength of a beam with its dimensions—length, breadth and depth (squared)—but did not attempt to relate it to the tensile strength of the material by means of a theory of bending. Perhaps because of the relatively high level of algebraic ability needed to use such formulae, full tables of beam sizes were published well into the 19th century. A later development, during the second half of the 18th century, was to incorporate an empirical constant into the formulae. This served as a pseudo-property, a characteristic of a particular material ('pseudo-' because it had no application outside the context of that particular formula). This practice also reflected some of the theoretical work which also had been going on in the 18th century, especially in France, in search of scientific theories to relate the strength of a beam to a genuine material property, the tensile strength of the material.

It was into a design community used to this type of well-established design procedure that a new material, cast iron, was dropped, so to speak, at the end of the 18th century. It immediately presented several new and severe problems:

- even its most fundamental property (tensile strength) was not known or familiar;
- like stone, but unlike timber and wrought iron (which was used only in small quantities, and nearly always in pure tension), cast iron was not equally strong in tension and compression (roughly six times stronger in compression); it was, therefore, not a material well suited to making beams, which involve equal magnitudes of tensile and compressive stresses;
- unlike traditional materials, artefacts of cast iron were made up to size, rather than cut down to size; it became, therefore, for the first time, most importance to establish the minimum quantity of material which could be used;

- the concept of 'relative strength' was of little use, since beams could be made in such an enormous variety of sizes and irregular cross-sections;
- it was not known to what extent the strength of the material and beams made out of it, varied with precise manufacturing details (chemical composition, quality of production etc.);
- not only was the material new, but it was also being used for many new applications: this meant new types of structure as well as unfamiliar loads, for example, the large beams of early steam engines, floor-beams supporting spinning machinery in multi-storey mills, and tram- and railway tracks and bridges.

Together, these many new aspects which faced the designers of beams led to a state of crisis. The immediate solution was an empirical one. Beams of many sizes and shapes were tested, mainly by the manufacturers themselves, and simple tables were compiled of beams as products which would serve particular needs. In order to verify that the strength of the beams was adequate, they were nearly always test-loaded up to a 'proof' load greater than the anticipated working load. This technique was satisfactory as far as the users went, but the manufacturers always knew that they had probably used more material than strictly necessary: the problem was, how much more? As the use of cast iron grew in the 1820s, it became increasingly desirable to be able to make better use of the material in order both to save material and to make lighter structures—much the same story as happened a century later with steel and which prompted the setting up of the Steel Structures Research Committee (see Chapter 11).

It was, then, only in the early 19th century that designers first really became interested in developing new ways of designing beams, ways which could determine the *absolute* strength of a beam. One of the earliest theoretical works in English dealing with the elastic behaviour of beams was the *Encyclopaedia Britannica* (1797) with articles by John Robison and Thomas Young. In 1803 Charles Bage based his practical design method for the floor-beams in his mills on this theory, and in doing so perpetuated the mistake which Robison (and Galileo) had made in placing the 'neutral axis' on the concave face of the beam. However, his method was nonetheless useful, since it also incorporated an adequate factor of safety, and he passed his method on to William Strutt, another designer of mills [Skempton 1956; Johnson & Skempton 1956/57; Hamilton 1940/41]. It is interesting to note that, even at this early stage in the development of cast iron beams, they accurately reflected both the bending moment diagram ('fish-belly' and 'turtle-back' girders, deeper at the centre of the span) and the material's different strengths in tension and compression (inverted T-sections), just as the Greeks had discovered, in other ways in their own time. In fact, as early as 1789, William Jessop had used a fish-bellied cast iron rail of roughly I-section, but it is unlikely that he used any calculations to determine its strength [Jewett 1967].

In 1822 Tredgold produced the first book for practical designers which incorporated the new methods based on the elastic theory of bending, and showed how to calculate both ultimate strengths and deflexions of beams using only the geometry of the cross-section and a value for the tensile strength of the material. He, too, made some theoretical errors in calculating the beam strength, but again these were masked by

an adequate safety factor. In addition he advocated designing according to a working stress (or sometimes a working strain, after the French manner). He thus diverted the designers' attention away from failure loads—a practice which was to continue for the next 130 years until the plastic design revolution.

As the spans of beams increased, and the inverted T-sections were reduced to their logical minimum using simple bending theory, so another effect became apparent—the lateral torsional buckling of the vertical web. It was Eaton Hodgkinson who solved this problem around 1830, after a great deal of experimenting and use of bending theory which we would recognize today, by adding a small flange to the top of the web to form the familiar shape of the modern I-section (albeit asymmetrical because it was developed for cast iron). The final stages of the story of the development of the modern I-beam had to wait only another few years or so when wrought iron started to replace cast iron for use in beams (following the problems with fatigue failures). Wrought iron was equally strong in tension and compression and was, by that time, being rolled into shape—both factors which favoured the use of regular sections. The wrought iron sections were, however, designed using the virtually same procedures as had been developed for cast iron beams.

It is particularly interesting to note that the dates of this revolution in beam design procedures occurred more than a hundred years *after* the corresponding scientific revolution (see §10.2). It was only as a result of the pressure from design engineers in the 1820s that Hodgkinson first put the beam theory of the late 17th century to good use in justifying the value of the I-section. Furthermore it was these design pressures which stimulated the new interest in bending theory which occupied engineering scientific community for the next century, although this subsequent work had relatively little effect on design procedures. The next major development in design procedures came with the introduction of reinforced concrete in the late 19th century and the plastic design revolution in the 1950s, both of which again were initiated by the engineering designers rather than engineering scientists.

13.2 ARCH AND VAULT DESIGN

One of the advantages of the masonry arch is that it is actually quite difficult to build an arch that will fall down. The intrados (under surface) can be of nearly any arch-like shape varying from a tall narrow pointed arch, any number of elliptical shapes to the elegant simplicity of the circular arch. The reason for this is two-fold. First, the shape of the intrados does not greatly affect the way in which the arch works as a structure. Secondly, the loads carried by most masonry arches are usually small in relation to the mass of the vault or bridge. Consequently, it has been possible to use all manner of design procedures with success, despite the fact that, as we would now say, their power to justify a proposed design was often based on utterly irrelevant logical arguments. It would be extremely difficult to persuade an Italian Renaissance designer that an arch bridge did not work *because* it was a *circular* arch—the designer could provide considerable evidence of successful bridges and vaults, while we would be hard-pressed to provide a single example of a circular arch bridge which did not work. The designer could equally argue that other shapes worked *because* they were made up of parts of circles, each of which worked in its own right. The predominance of the

circular arch would seem, then, to have its explanation in certain beliefs about the circle, for instance, that it was one of the shapes favoured by the creator of the universe, especially as manifested in the perfection of the geometrical shape itself.

After a millennium of circular arches in Europe, the Gothic period saw the introduction of the pointed arch. It can now only be a matter of speculation as to whether it is more than a coincidence that the source of this idea is widely supposed to have been the Saracen world of the Moors and Arabs, the same culture which brought Euclid's geometry to the attention of mediaeval France. Certainly the pointed arch was better suited to the tall lightweight slenderness of Gothic structures as it exerts a smaller lateral thrust and allows the voussoirs to be less thick than in a circular arch.

The first evidence we have of particular design procedures for arches are those for the window arches and rib vaults of cathedrals. The familiar Gothic pointed arch could be generated with two circular arcs in several different ways according to the desired span to height ratio (Fig. 13.1a–c). A further geometrical step could be taken to establish the thickness of a pointed arch (Fig. 13.1d [Bucher 1972a]) although this particular method (c.1480) is rather conservative (1:5 thickness:span ratio) and would have been of little use for other types of arch since it suggests the need for thicker arch ribs as the arch becomes more pointed, in direct contrast to what is most sensible.

For bridges and vaults, both of which are described as consisting of 'arches and stuffing', Alberti recommends the semicircular shape as 'the strongest of all the arches', although he only considers circular arches varying from a full semi-circle ('entire') down to sectors ('imperfect' or 'diminished') of about 100 degrees and pointed ('composite') arches comprising two circular arcs. He justifies this belief (in the sometimes shaky 1755 translation) as being so:

> not only from experience, but reason; for I do not see how it can possibly disunite of itself, unless one wedge shoves out another, which they are so far from doing, that they assist and support one another. And indeed, if they were to go about any such violence, they would be prevented by the very nature of ponderosity, by which they are pressed downwards, either by some superstructure, or by that which is in the wedges themselves. This makes Varro say, that in arches, *the work on the right hand is kept up no less by that on the left, than the work on the left is by that on the right*. And if we look only into the thing itself; how is it possible for the middle wedge at top, which is the key stone to the whole, to thrust out either of the two next side wedges, or how can that be driven out of its place by them? The next wedges also in the turn of the arch, being justly counterpoised, will surely stand to their duty, and lastly how can the two wedges under the two feet of the arch, ever be moved while the upper ones stand firm? Therefore we have no need of a chord, or bar in an entire arch because it supports itself by its own strength; but in diminished arches there is occasion for an iron chain or bar, or for an extension of wall on both sides to supply the want of strength. [Alberti 1485:57]

While his argument relies rather excessively on the contribution made by friction, it shows an attempt at providing a physical explanation to back up experience and the (to Alberti) philosophical truth that circles were best *because* they were circles. He, and Varro (a contemporary of Vitruvius), would also appear to be aware (in the italicized sentence) of the stabilizing effect of the self-weight of each side of a

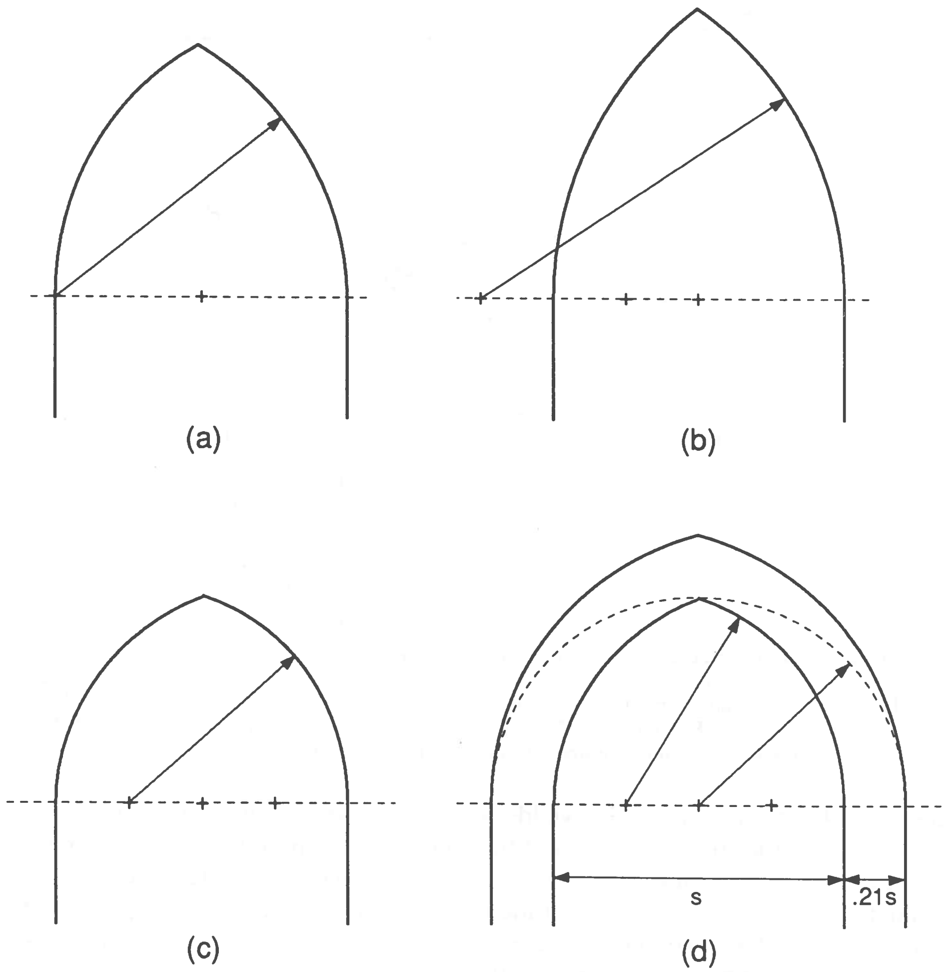


Fig. 13.1 (a)–(c) Design procedures for pointed (Gothic) arches giving different span:rise ratios; (d) a simple design procedure used for generating arch rib thickness from arch geometry

voussoir arch and its 'stuffing' which prevent the establishment of a failure mechanism due to a point load on the other half.†

The side thrust due to an arch was of rather greater concern in the case of masonry vaults than in bridges, since such a force tends to overturn the wall from which the arch springs. It is interesting to find several geometric design procedures which go a considerable way to being able to take account of the factors which affect the stability of a vault thrusting onto a wall or abutment. From the mid-16th century we know of five different methods which the Spanish architect Rodrigo Gil de Hontañon collected and perhaps used [Gil c.1540; Kubler 1944]. Each method gives different

† Alberti's words were 'Hinc illud Varronic, quod sit in arcuatis operibus dextra no magis stare ex sinistris, quam sinistra ex dextris'.

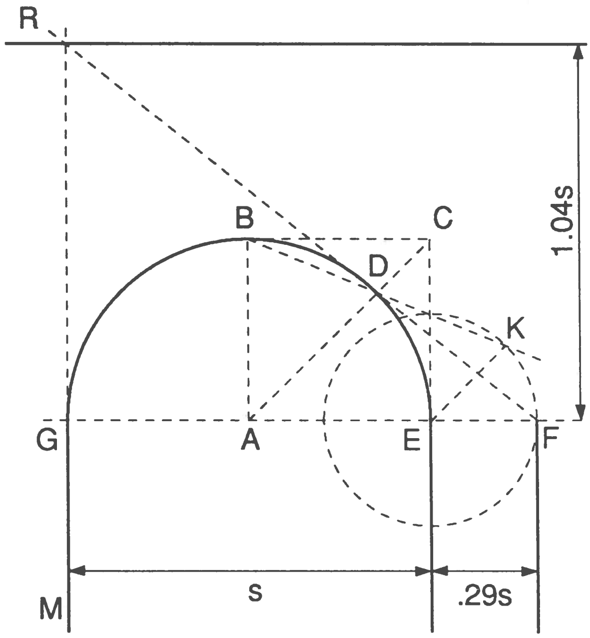


Fig. 13.2—Design procedure for establishing the required thickness of abutment and height of superimposed masonry which a semi-circular arch rib can carry, after Rodrigo Gil de Hontañon. (D is the intersection of the arch and AC; produce BD to cut EK parallel to AC. The circle, radius EK, gives the abutment width, EF. The intersection of FD and MG, produced, determines R and the height of masonry which can be supported)

answers! One numerical method yields an abutment whose thickness varies with the square root of the sum of the span of the vault, the length of the structural bay and the height of the wall on which the vault rests. Although not very reliable by modern standards, the required size of abutment does, to a certain extent, depend on the right variables. Three geometric methods show how to derive both the thickness of the abutment required for a semi-circular arch and the weight of masonry such an arch could support (see Figs 13.2, 13.3 and 13.4). The most sophisticated of the geometrical design procedures shows how the span of the arch, the thickness of the arch rib and both the thickness and height of the abutment are all inter-related for both a semi-circular and a pointed arch. Elsewhere, when talking of a second numerical design procedure, he observes that 'it is the most accurate ("*cercano*" = close) and rational of all of the rules', even though he acknowledges that the result is different from the one Vitruvius recommends.

There were many other geometric methods for establishing the thickness of abutments for various types of arched vaults and even the height to which the abutment should rise above the level of the springing of the arch (Fig. 13.5, [Walter 1766]). The most famous of the abutment design procedures, Blondel's rule, originated in the early 17th century and could be applied to any shape of arch as long as the abutment was less than 1.5 times higher than the arch span (Fig. 13.6; see also Fig. 13.5). The procedure was later widely publicized in the most famous engineering text book of its day [Bélidor 1729] and it has been reported to have appeared in print as a useful design tool even in the present century [Straub 1952:92].

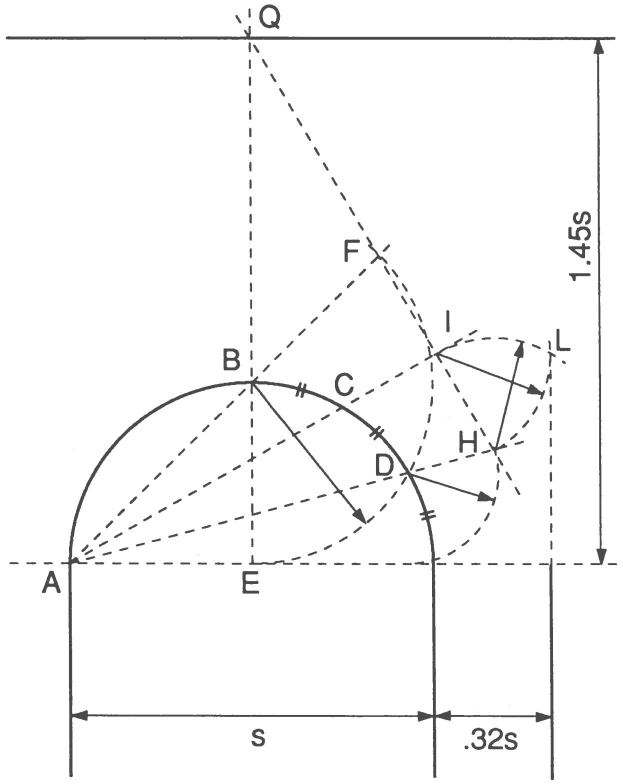


Fig. 13.3—A second design procedure for establishing the required thickness of abutment and height of superimposed masonry which a semi-circular arch rib can carry, after Rodrigo Gil de Hontañon. (C and D trisect the quadrant. AB, produced, intersects circle, radius BE, in F; AD, produced, intersects the circle centred at D, in H. AC, produced, intersects FH in I. The equilateral triangle on base IH gives L, from which a perpendicular establishes the abutment width. Q, the intersection of EB and HF, produced, establishes the height of masonry which can be supported)

Of particular interest in Gil's writings is his dissatisfaction with the all the methods for sizing abutments which he has come across:

> Probado hé muchas veces á sacar razon del estribo que habrá menester una cualquiera forma y nunca hallo regla que me sea suficiente, y tambien lo he probado entre los arquitectos españoles y extranjeros, y ninguno paresce alcanzar verificada regla, mas de su solo albedrio; y preguntando por qué sabrémos ser aquello bastante estribo, se responde porque lo há menester, mas no por qué razon. Unos le dan el ¼ y otros, por ciertas lineas ortogonales lo hacen y se osan encomendar á ello, teniéndolo por firme.† [Gil in Mariátegui 1868:175]

† I have often tried to work out the size of abutment for any given size of arch and I have never found a rule which was adequate for me. I have enquired of other architects, both Spanish and foriegn, and none seems to have managed to verify a rule other than by his own whim; and upon asking how we might know whether a certain abutment is enough, they reply that it was needed, but not for what reason. Some say a quarter [of the span] while others make certain geometrical constructions and dare to recommend them, believing them to be correct.

We certainly see a strong wish to have design procedures which were based upon reason, and a tone of voice which implies that Gil was not convinced by their ability to justify his designs. More than a century later a similar dissatisfaction was expressed by Bélidor about the rationality of Blondel's arch design procedure which, he

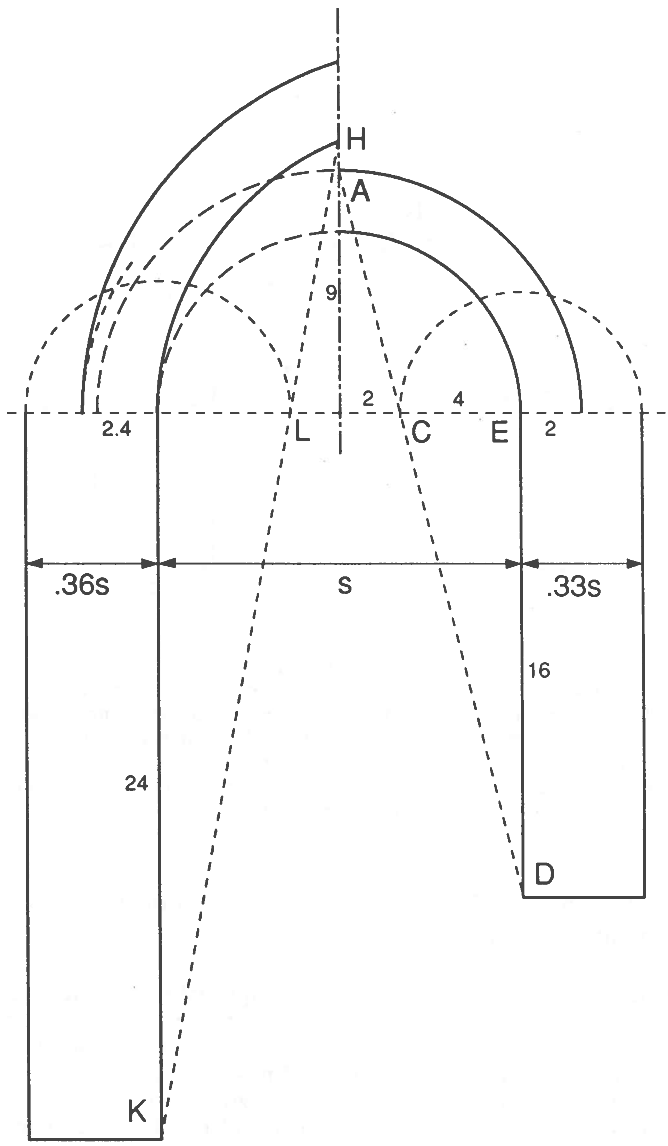


Fig. 13.4—A third design procedure, for establishing the thickness of the arch rib, and the height and thickness of abutment for both a semi-circular and a pointed arch rib, after Rodrigo Gil de Hontañon. (For a semi-circular arch (of 12 feet span in Gil's example), the rib thickness is one-sixth, and the abutment thickness, one-third of the span; AC, produced, gives the abutment height, ED. For a pointed arch of rise:half-span=3:2, the rib thickness is one fifth greater, and the abutment height is greater in proportion to the height of the pointed arch (i.e. 3:2), than for a semi-circular rib; the abutment width is established by where HK cuts the span, at L)

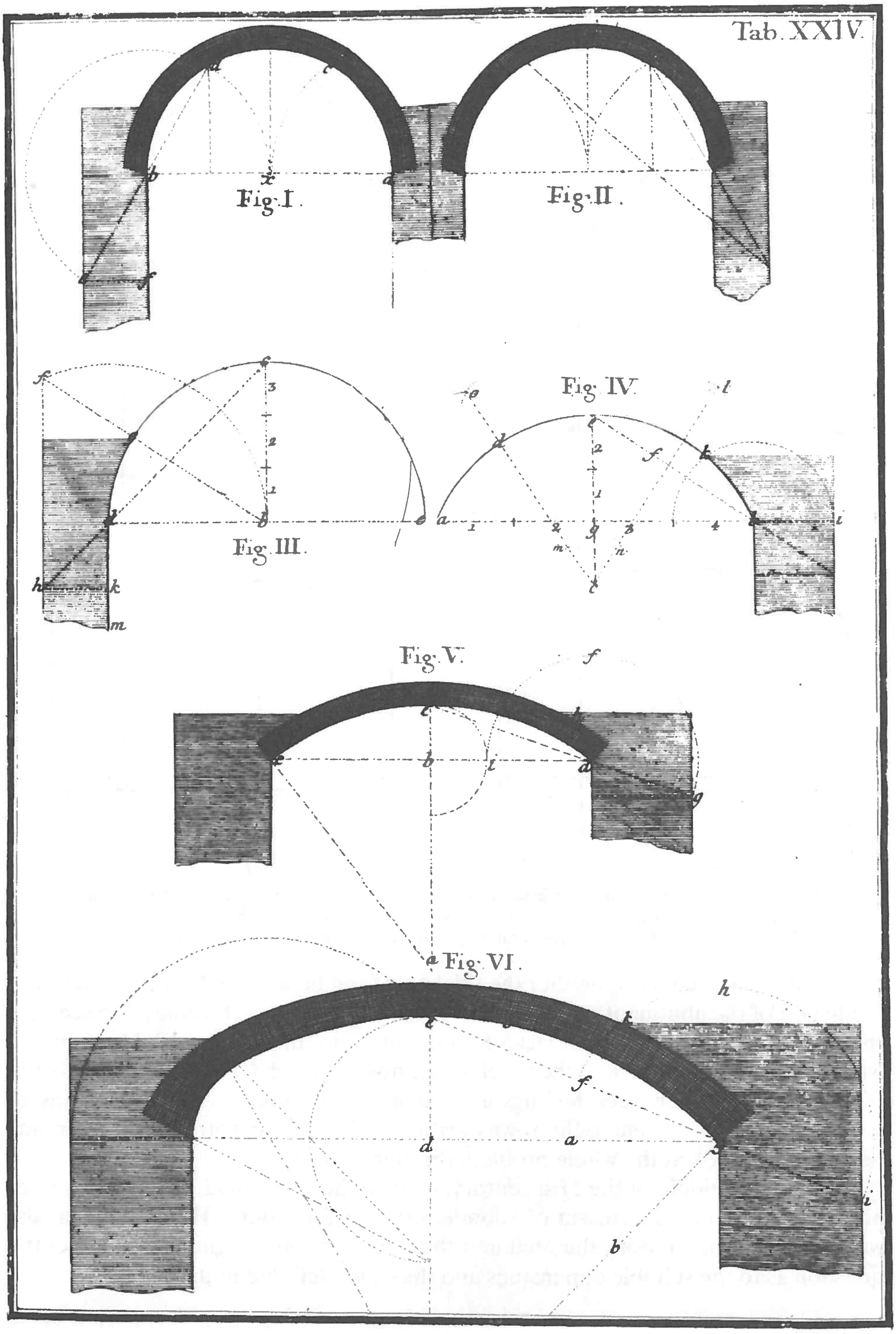


Fig. 13.5—Several design procedures for establishing the abutment width and height above the springing for masonry vaults. [Walter 1766]

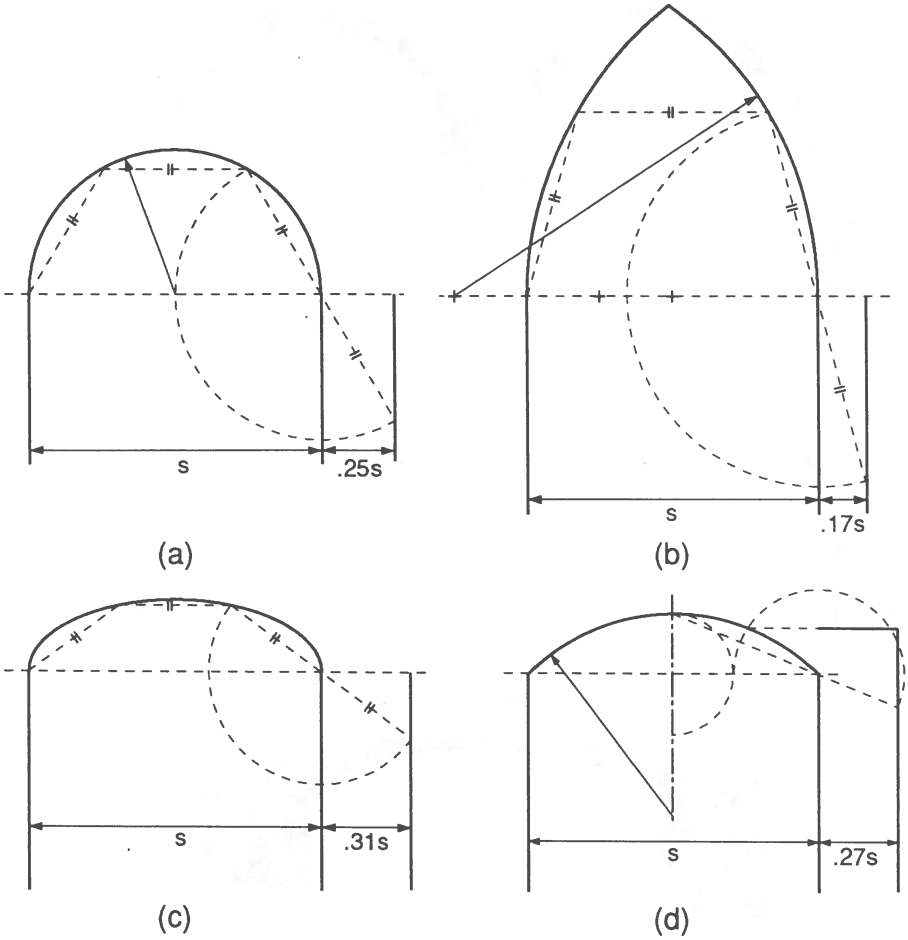


Fig. 13.6—Design procedures for establishing abutment width for various arches. Blondel's procedure applied to (a) a semi-circular arch (see Fig. I in Fig. 13.5), (b) a pointed arch, (c) a flat arch; (d) another design procedure for flat arches (see Walter's Fig. V in Fig. 13.5)

observed, took account of neither the thickness (and hence weight) of the arch, nor the height of the abutment (and hence its stability against overturning). We see here the same sort of concern about lack of rationality, and, therefore, inability to justify, which aroused the concern of the Steel Structures Research Committee in the 1930s. It is tempting to view such feelings as evidence of Kuhnian 'anomaly' leading to 'crisis'. In fact, by the time Bélidor was writing (1729), the first attempts had already been made to look at the whole problem through new eyes.

Around the middle of the 17th century, Christopher Wren had, like Bélidor after him, made a detailed criticism of Blondel's design procedure. He argues that the weight of material in both the arch and the abutment must significantly affect the question as to the suitable dimensions and thus Blondel's method:

> is neither true nor universal: and what is true will be shown to be only determinable by the doctrine of finding the centres of gravity in the parts of the proposed design. (Cited in [Hamilton 1933/34:34])

In his notebook Wren puts forward the requirement that the masses of masonry in the arch and the abutment must counter-balance one another (Fig. 13.7). This approach, which takes no account of how the arch works, should not, however, be seen as evidence of Wren's ignorance. A glance at any of his buildings, especially the remarkable stucture of his dome over St Paul's Cathedral, will show that he fully understood the tendancy of arches and domes to thrust outwards. Elsewhere he considers the problem of how to support one foot of an arch on a column 'so small as to be unable to be a sufficient butment to the pressure of the arch'. He then says he can add weight to the column above the level of the arch 'to establish it so firm as if I had [built] a butment' and continues:

> This is the reason why, in all Gothick fabricks of this form, the architects were wont to build towers or steeples in the middle, not only for ornament, but to confirm the middle pillars against the thrust of the several rows of arches, which force against them every way. cited in [Frankl 1960:366]

Wren is clear that the purpose of his proposal to establish the positions of the centres of gravity of parts of the structure is to assist with the *design* of an arch. It would, therefore, seem likely that his requirement was to devise a way of calculating the required dimensions (the 'description') while also rationally incorporating the weights of the abutment and (semi-)arch (an example of a 'grounded rule', see §6.1.3). This would have met his criticism of Blondel's method and provided a more rational 'justification' of a proposed design than the discredited, purely geometric

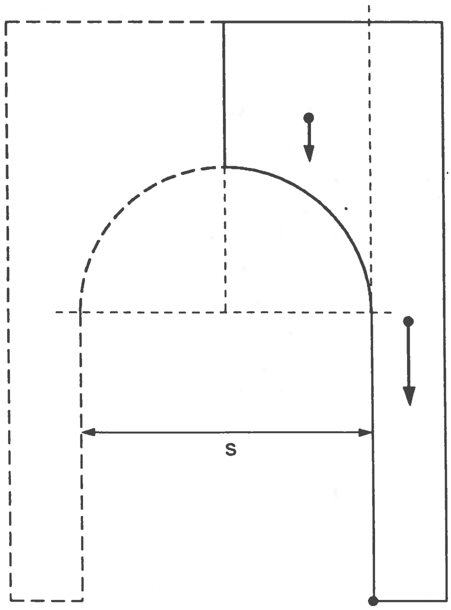


Fig. 13.7—Wren's design procedure for masonry arch and abutment: the greater the superimposed load, the wider the abutment must be. Equilibrium about the inner edge of the abutment is considered.

method. We should remember that Wren was writing several years before his friend, Robert Hooke, was to record for the first time that the ideal shape for an arch was the inversion of the correspondingly weighted hanging chain, and it is possible that Wren used this method to help determine the shape of the dome of St Paul's.† But it was still a further twenty years before the first useful analytical treatment of the statical equilibrium of the arch appeared.

In 1695 Lahire adopted a purely statical approach which showed that a semi-circular arch in which there is no friction between the voussoirs, can only be in equilibrium if the lower voussoirs approach infinite weight—in other words it cannot stand. Rather than proceed to establish the shape of arch will stand (Hooke's inverted chain), which would have been a useful conclusion, he rejects the approach as a reliable design procedure (although its descendants continued well into the 19th century as the 'equilibrated arch'). Lahire proposed a second approach to the problem (in 1712) was of more practical use and was based on the well-known fact that arches tend to rupture at a point part way between the springing and the crown. Each half of the arch can be treated as two monoliths which must be in equilibrium, and he shows that the arch on its abutment is stable if the overturning moment due to the weight of the upper part is less than the restoring moment due to the weight of the abutment and lower part of the arch. This latter analysis was expressed in a rather mathematical way and unlikely to appeal to the engineer. It fell to Bélidor to simplify and adapt Lahire's analysis to give a practical design procedure (Fig. 13.8). Nevertheless, Bélidor does not particularly commend this approach to his readers and still implies a preference for Blondel's design procedure which had already proven its reliability. We have to remember that throughout 18th-century France, buildings and bridges were conceived of in terms of their *solidité* [Picon 1988:163ff]. This concept was a sort of half-way house between the purely non-statical concepts of stability, and those which involved concepts of statics such as weight, force and overturning. A structure which possessed it was considered a better one than another which lacked it, by the client, the designer and the builder. By comparison with a structure possessing *solidité*, a structure designed to be in equilibrium (using statics) was considered to be somewhat delicate—perhaps because, even to think of equilibrium involved considering the possibility of overturning and collapse‡.

Lahire's second method, in which he considered the possible collapse of the arch (and sought to prevent it) was utterly different from the other which considered equilibrium but not stability. This approach to the stability of arches was rediscovered over and over again during the following century or so by such names as Coulomb, Couplet, Poleni, Lamé and Clapéyron, each time to be forgotten again; forgotten, that is, until the recent reappraisal of the history of arch design and analysis following the plastic design revolution which also relies on a consideration of collapse mechanisms.

The history following this major revolution in the design of arches and vaults is large and complex and has been well covered elsewhere [e.g. Heyman 1972; 1982;

† To thwart plagiarism Hooke published it in 1676 as a Latin anagram: abcccddeeeeefggiiiiiiiillmmmmnnnnnooprrssstttttttuuuuuuuuux—*ut pendet continuum flexile, sic stabit continuum rigidum inversum.* His more famous law appeared as ceiiinosssttuv—*ut tensio sic vis*.

‡ Baker came across the same attitude in elastic design engineers towards the very idea of contemplating collapse which formed part of the plastic design procedures he was advocating (see p.109).

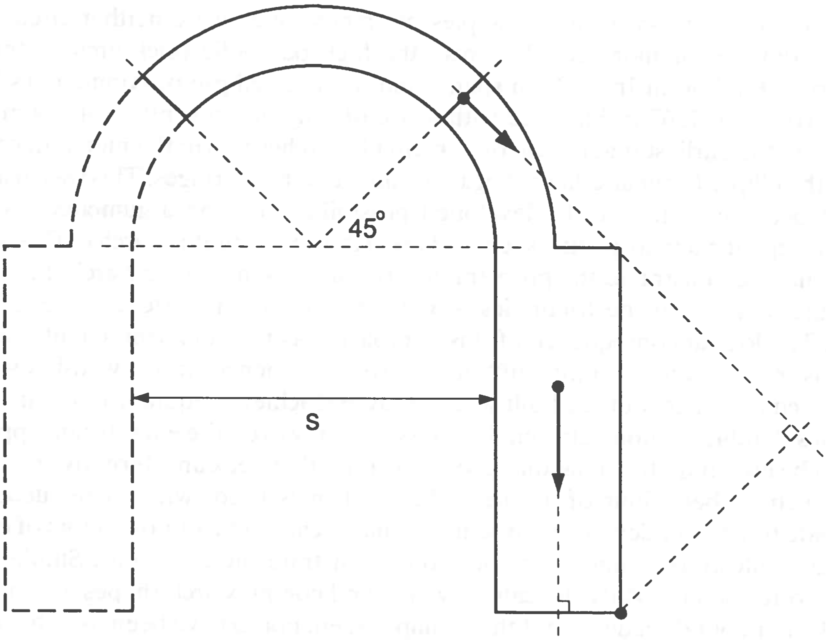


Fig. 13.8—Bélidor's simplified version of Lahire's design procedure for masonry arch and abutment. Equilibrium of arch thrust and abutment weight about the outer edge of the abutment is considered.

Ruddock 1979]. It was to take many more years for the purely geometric design procedures to lose favour despite the enormous amount of experimental and theoretical work carried out throughout the 18th century both on arch equilibrium and stability. In the absence of engineers' design calculations it is difficult to assess the impact which the statical revolution had upon the design of arches. Considering the continuing references to Blondel's and other geometric procedures [e.g. Walter 1766] it is more than likely that they and statical design procedures (based on Lahire and Coulomb's work) lived side by side throughout the 18th century, the geometric ones, perhaps, being preferred by those who did not understand statics. Matters were to take a different turn at the beginning of the 19th century when another scientific revolution was beginning to influence all engineering design procedures—the 'elastic' revolution. This revolution had derived from investigations into the strength, and later the stiffness, of beams (see Chapter 10) and transformed the world of the engineering designer. From the 1820s to the present day, the main concern of the designer has been the level of stress inside the various materials of which stuctures are made.†

† It is rather surprising that this elastic approach has come to be applied widely in assessing the safety of arches, especially of masonry, since stresses in masonry structures seldom exceed 10% of the fracture strength. It is equally surprising that, even 30 years after the latest revival of interest in the purely statical and geometric approach to arch design (limit state design), people persist in using the somewhat discredited elastic design procedures. There would appear to be the same reluctance to abandon the well-established elastic methods in favour of more rational ones which we see in Bélidor's persistence with Blondel's purely geometric procedures.

There had been occasional examples of arches which were neither circular nor made from two or more circular arcs—the high parabolic brick arch of the 3rd-century Ctesiphon in Iraq (25 m span), and the near-ellipse of Ammanati's Ponte Santa Trinita of 1567 in Florence with a rise of only one-seventh of its 29 m span. However, the earliest regular use of non-circular arches was in the mid-18th century when the ellipse (or near-ellipse) began to gain favour for bridges. This was to a large extent because of the newly developed possibility of using arguments based on statical equilibrium to justify such a shape (the 'equilibrated' arch). This use of statics had demonstrated the problem of proposing a semi-circular arch if the voussoirs are assumed to be frictionless—abutments of infinite size are needed (Fig. 13.9). The logical consequence of this approach was to avoid semi-circular arches. Yet this solution was a triumph of 'theory' over experience. It was well-known that semi-circular arches could be built successfully and achieved equilibrium by means of the considerable friction between the voussoirs. However, the equilibrium approach cannot be used if friction is assumed to act. Rather than seek an alternative model for the structural behaviour of an arch, the academics tried, with some success, to persuade bridge builders to avoid semi-circular arches in favour of sectors of circles. Fig. 13.10 illustrates some tests done to demonstrate the argument. Similar arguments were used to justify the catenary arch and complex arch shapes which better suited a horizontal roadway, but these shapes seem not to have been used by designers [Ruddock 1979]. In practice, arches with a vertical intrados at the abutments were frequently used, despite the logical case against them; common sense had prevailed. Non-statical issues such as visual appearance, the height of the roadway above the water, the clearance for river traffic and flood water beneath the arch and the ease of setting out and constructing the arch still had the over-riding influence. It was also usual to design low-rise arches as several segments of circular arches rather than true ellipses. This facilitated the manufacture and erection of the centering, and avoided the need for a different template for each voussoir of the arch. Cresy devotes no less than ten pages to the geometry of such arches which could comprise from 3 up to 13 circular arcs (Fig. 13.11). He pointed out that such shapes could be justified using Emerson's Fluxions (calculus) or Hutton's Principles of Bridges by the reader 'who is desirous of becoming acquainted with these delicate but important applications of mathematical science' [Cresy 1865:1487]. He also recommends the design procedure to establish the shape of equilibrium which involved hanging up a chain of weights representing the voussoirs. For the most part, however, he recommends following the precedent established for certain shapes of arch based on experience.

Arches which accurately reflect the structural actions involved—the funicular shape for dead loads and the need for stiffening to withstand asymmetric live loading—only made their appearance in the 1870s after the development of graphical statics by Culmann and Schwedler in the mid-1860s. This technique enabled designers to consider both axial forces and bending moments in complex structures for which numerical analytical methods would have been too cumbersome. Statical equilibrium and bending moment diagrams became a standard part of design procedures for the first time and influenced designers to propose new types of arch structure which reflected the use of the newly developed design tools. Schwedler built the first 3-pin arches in 1865–66 in which the thickness of the two arch segments clearly reflect the bending they must withstand. In the following decade we see Eiffel using

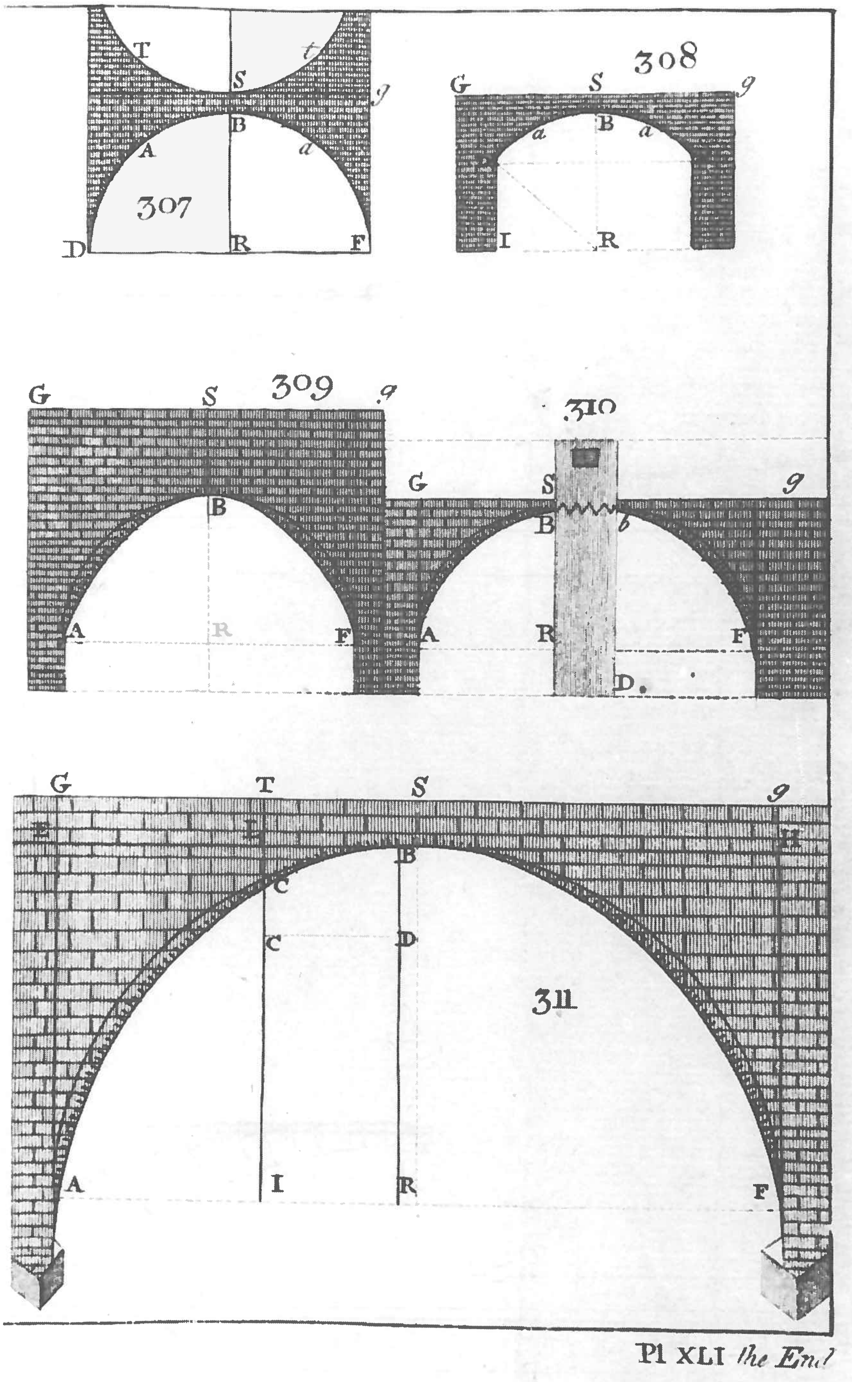


Fig. 13.9—Theoretical design for an 'equilibrated' arch leading to the inconvenient requirement for voussoirs of infinite size for an arch which rises vertically at the springings (such as a semi-circular arch, #307). Hence the various arches, #308–311, which are either non-circular or spring at an angle [Emerson 1758]

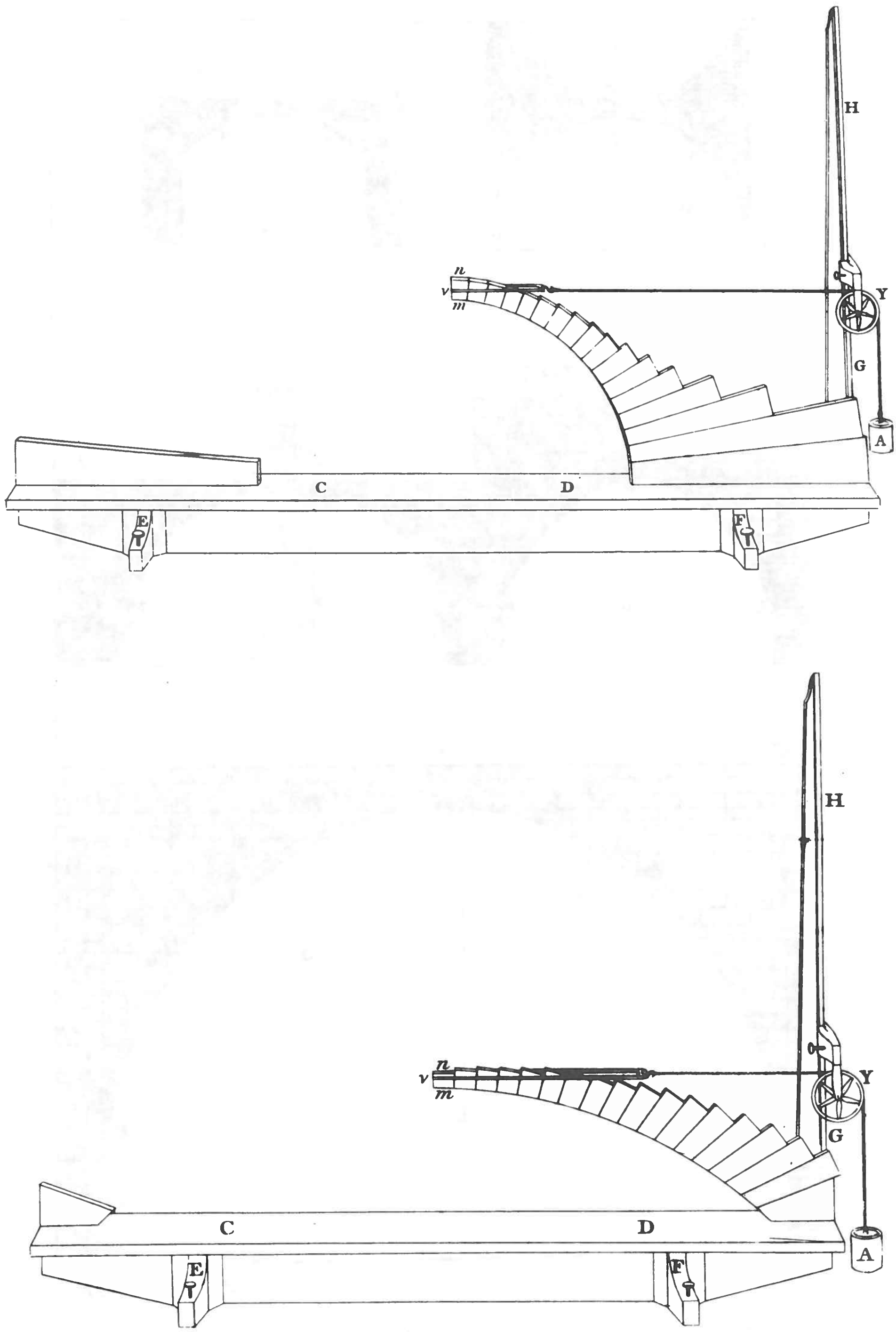


Fig. 13.10—Demonstration, based on equilibrium and 'frictionless' voussoirs, of the practical advantages of reduced arches (having realistically sized voussoirs), and of the greater thrust they exert on the abutments (the loads marked 'A') [Attwood 1801]

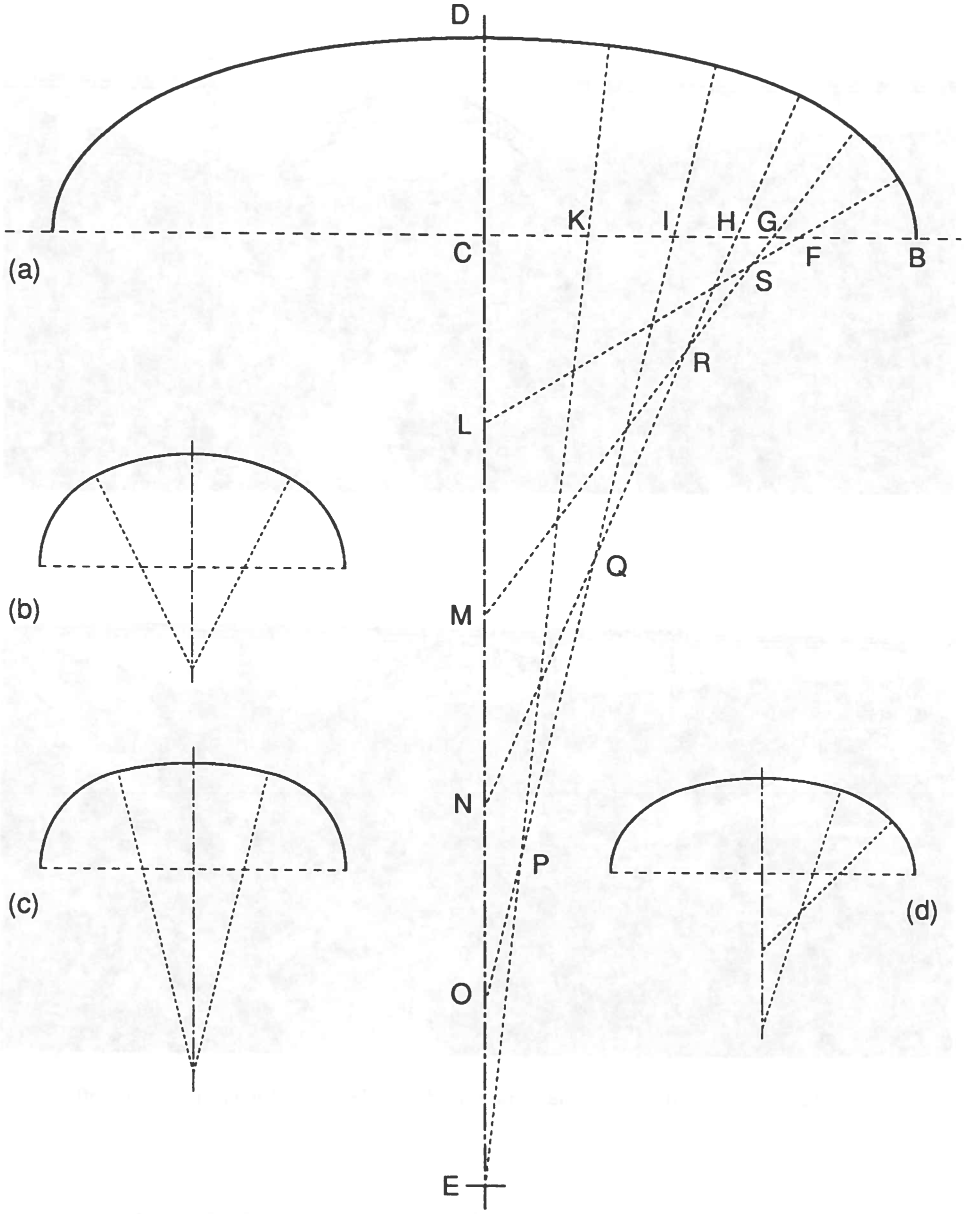
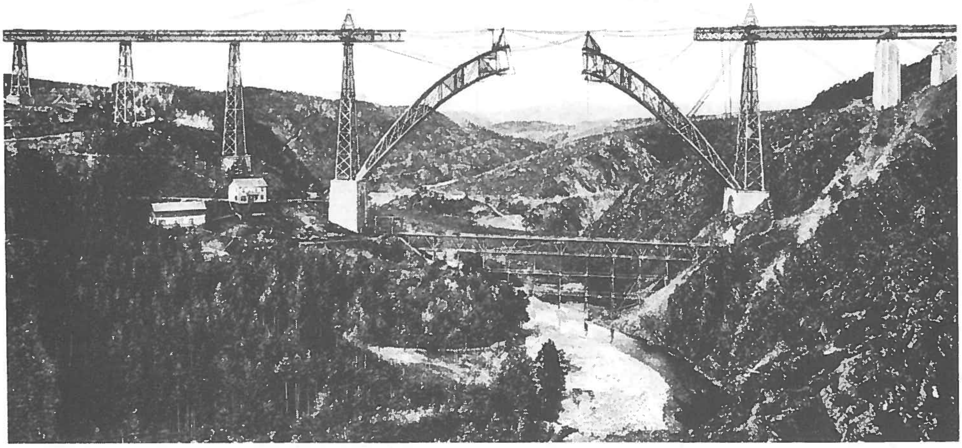


Fig. 13.11—Design procedures for establishing a number of many-centred arches. The 11-centred arch (a) dates from the mid-19th century. Choose the half span BC, rise CD and curvature at springing, FG. Set CE as, say, 3×CF. Divide CE into five equal parts. Divide CF into five parts in the ratio 5:4:3:2:1. The centres are established at E, P, Q, R, S and F. For many flat arches the less tedious 5-centred arch (d) was generally considered satisfactory. In Gothic designs, various 3-centred arches were sometimes used (b and c)



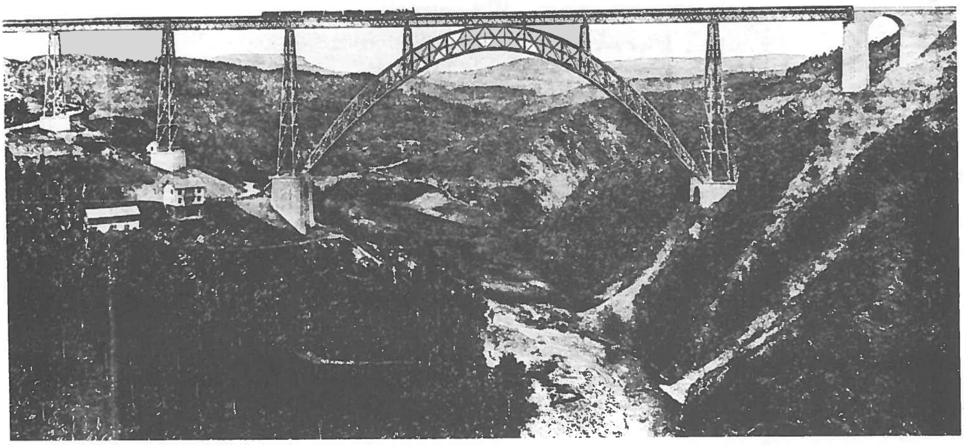


Fig. 13.12—The crescent shape of the Garabit viaduct 1879–1884. (Engineer: Gustav Eiffel)

large 2-pin arches for his Douro and Garabit viaducts (begun 1875 and 1879, respectively) which again show a form reflecting the bending the arch must resist as well as pinned supports to render the design more easily justifiable by calculation (Fig. 13.12). The true funicular shape of the arch could only be expressed when the effect of asymmetric live loads could be resisted by a stiff bridge deck. This was not practicable in the case of heavy railway locomotives and its first appearance had to wait for road bridges and the Maillart's development of reinforced concrete in the early 20th century. This structurally, slightly dishonest solution was not the result of a design revolution—rather, the elegant use of a material to its very best advantage (Fig. 13.13).

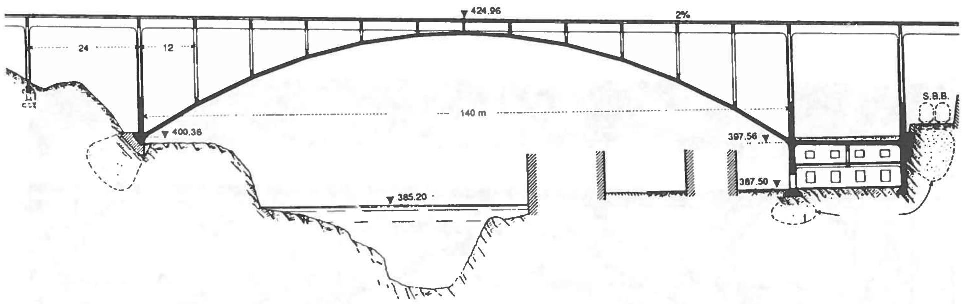


Fig. 13.13—Funicular reinforced-concrete arch by Robert Maillart: project for a bridge over the Rhine at Schaffhausen, 1935 [Bill 1949:108]

13.3 TRUSS BRIDGE DESIGN

The development of the design of truss bridges up to the end of the last century is particularly interesting as it constitutes one of the few examples from the history of structural design in which evidence of the type of justification used in the design procedure can often be seen in the finished artefact. It therefore makes the various revolutions in design procedure more visible than was the case with beam or arch design. In the following brief resume of the subject, eight stages of development of the design procedures are identified.

Traditional trusses

Up to the middle of the 18th century, the design of trusses had been what is now usually called 'empirical'. The designs still often reflected the simplest of bridge types—a beam propped by diagonals rising from each river bank. The timber was used in compression wherever possible since joints to carry significant tension forces were difficult and expensive to make and impracticable to maintain. For medium and long spans the overall disposition of the structural timbers therefore tends to follow the general form of an arch (rising in the centre) to keep as many members as possible in compression (Figs. 13.14 and 13.15). However, they were clearly not seen as working in the manner of the familiar masonry arch. From the intricate interconnections between timbers and often nearly random manner in which they are arranged, there is also little doubt that no-one was trying to simplify the structure in order to make it amenable to any type of mathematical modelling. Even today such bridges are impossible to analyse with any degree of confidence.

The justification of proposed bridge designs would have relied mainly on experience and precedent and would have been one of the skills of the craftsmen who made them. There is some evidence that geometric design procedures were used for timber truss bridges. One early 18th-century German book, for instance, illustrates elaborate geometric design procedures based on various subdivisions of a circle drawn with the bridge deck as diameter [Werner 1980:75]. Later books in English would indicate that these methods did not travel across the Channel. In the case of large bridges, models were often made, but this was usually to help to plan the construction sequence. Structural testing of models was not unknown though. Walter reports tests he

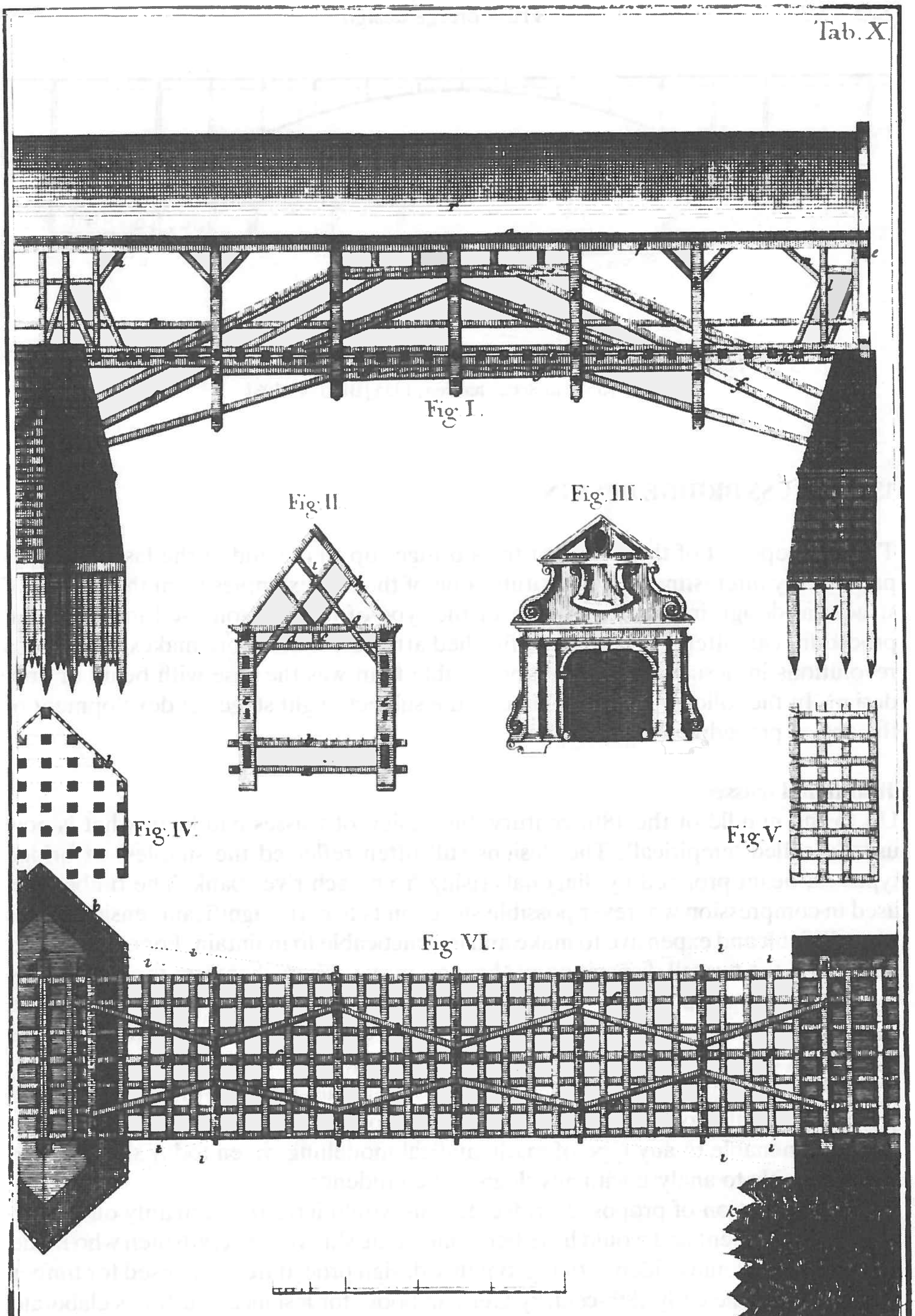


Fig. 13.14—100-foot timber truss combining mainly beam action and 'propping'. The only tension members are the verticals (called *'Hängesäulen'*—hanging columns) which work in the manner of the Queen posts in a roof truss [Walter 1766]

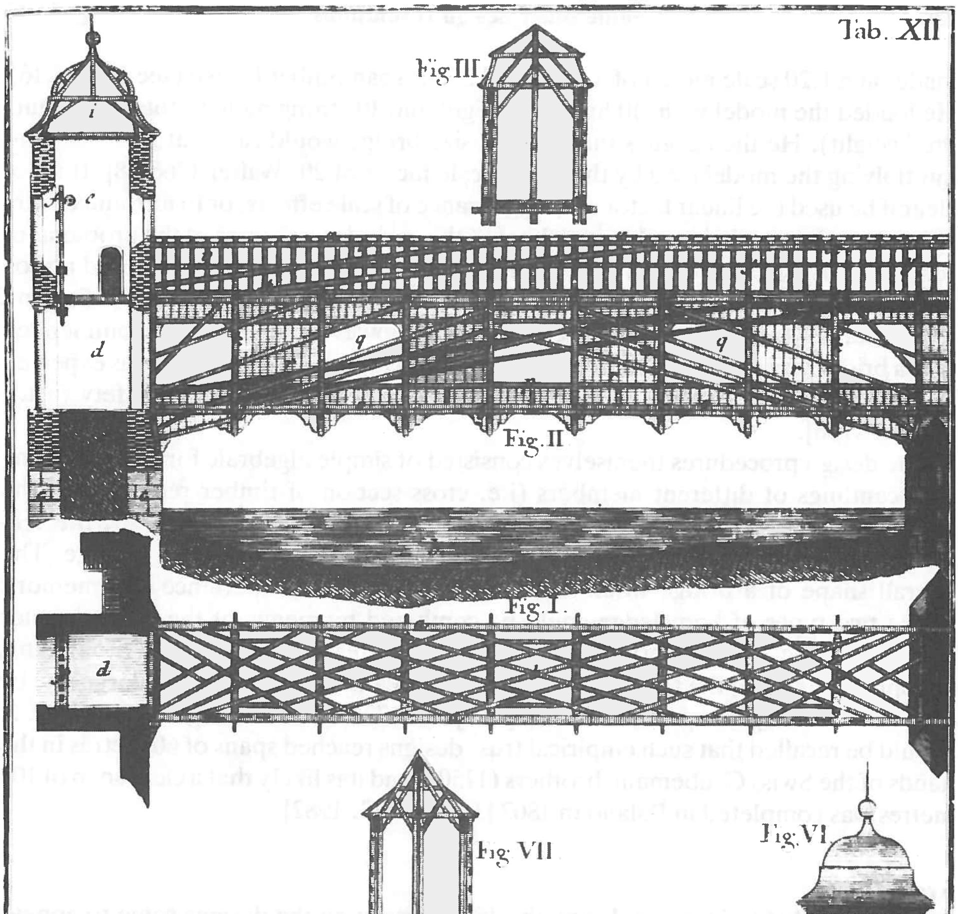


Fig. 13.15—Alternative schemes for a timber bridge comprising two 170-foot spans. Clearance above the water was increased by keeping all the compression members within the depth of the walkway. The members spanning the full 340 feet, which resemble very flat arches, can have had little beneficial effect [Walter 1766]

made on a 1:20 scale model of a proposed 250-ft span timber bridge (see Fig. 13.16). He loaded the model with '10 hundredweight and 10 strong men' (a total of 25 hundredweight). He then argues that the full-size bridge would carry 'at least' 25 tons, multiplying the model load by the linear scale factor of 20 [Walter 1766:28]. It is not clear if he used the linear factor out of ignorance of scale effects, or to introduce extra safety into the prediction. Other authors of the period were aware of the problems of scaling loads and dimensions between real and model structures, but offered no solutions [Hauri 1979]. Many completed bridges were tested, mainly to check deflexions, of course, not to establish their strength. It was, however, sometimes recommended that a bridge was loaded a certain number of times more severely than it was expected to sustain in use. This would have established a genuine minimum safety factor [Walter 1766].

The design procedures themselves consisted of simple algebraic formulae relating the scantlings of different members (i.e. cross-section of timber required) to the likely imposed load, the type of timber and span of the river. Curiously, the self-weight of the bridge was never included, even though it could be very large. The overall shape of a bridge would have been known from experience and memory. These two types of knowledge could be combined by means of the rather vaguer concept of the general 'principles' underlying different bridge designs—arching, propping, tying and the joining of several small beams to create a single large one by means of scarfing (to prevent shear) [Leupold 1726, Walter 1766, Reuss 1789]. It should be recalled that such empirical truss designs reached spans of 60 metres in the hands of the Swiss Grubemann brothers (1750s) and it is likely that a clear span of 102 metres was completed in Poland in 1802 [James J. G. 1982].

Arch-trusses

As spans tended to increase during the 18th century, so the designs came to appear more arch-like and this finally came to the structure being *rationalized* as a relatively

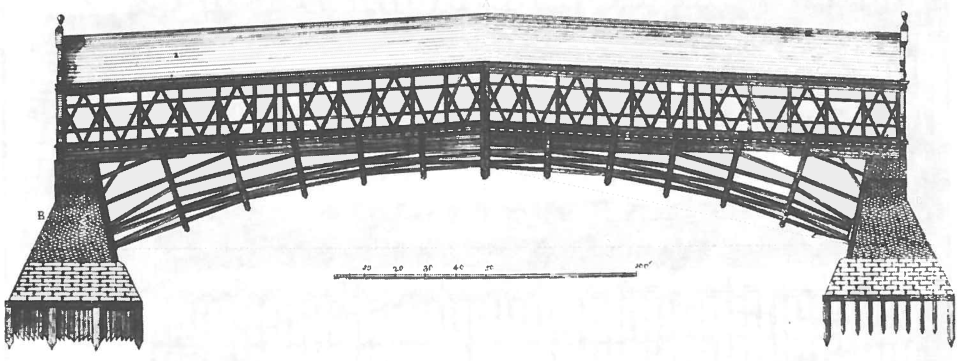


Fig. 13.16—Scheme for a 250-foot timber arch (or arch-truss) bridge. Walter built a 1:20 scale model and loaded it with '10 hundredweight and 10 strong men' to justify the design [Walter 1766]

pure arch stiffened (braced) by the addition of rather few struts. They can hardly be called trusses in the modern sense, but one must remember that 'truss' originally meant to provide additional support to a beam, both to strengthen and to stiffen it. I use the word 'rationalize' deliberately to indicate the idea of seeing the bridge as a *structural arch*, rather than as being merely 'arch-shaped'. A structural arch was, by that time, capable of being understood in terms of the statics of arch equilibrium and this approach was being widely used by late 18th-century designers of masonry arch bridges (the 'equilibrated' arch). This approach, albeit applied to an arch made of a different material, could at least allow some attempt to be made at estimating the horizontal thrust exerted by an arch on its abutments. Concomitant with choosing to see the bridge (figuratively) as a structural arch, was the decision to construct it as one (literally), i.e. to construct an independent arch of timber and to stiffen it (empirically) by means of a certain amount of bracing. This approach was taken by many designers during the late 18th and early 19th century. Wiebeking, who built timber arch spans of up to 70 metres and proposed one span of 180 metres, specifically mentions the use of arch theory to help justify the cross-section of timber required for the load-bearing arches, but this theory was of no use in justifying the scantlings of the bracing members required to stiffen the arches which, he says, can be determined only from experience (see also [Späth 1811]). Incidentally, one of Wiebeking's early bridges (Pont Caroline), built before he had developed the clarity of his structural arches, nicely expresses his ambiguous attitude to the arch-truss by hiding a traditional, somewhat arch-shaped, timber truss bridge (not a structural arch) behind a timber facade which made it look from a distance like a masonry arch [Wiebeking 1810].

Cross-braced and lattice trusses

The third stage of development of bridge trusses spanned roughly the period between the 1820s and the 1860s. By this time, the elastic theory of beams had become highly developed (especially due to Navier) and the possibility of using it to justify the designs of highly redundant cross-braced and lattice truss bridges was exploited. This was done by treating the bridge as a beam of similar overall dimensions and modifying the results according to the proportion of a solid-sided beam which was missing in the truss [Navier 1826; Ghega 1845]. The correspondence between the method of justification and the chosen design is manifested during this period by the fact that many truss and lattice bridges were built with parallel top and bottom chords, the better to resemble the beam shape which the mathematical model was most easily able to represent in the process of justifying the design. Despite the highly approximate nature of this type of mathematical model, it did result in a good understanding of the shear forces in the cross-braced or lattice 'web' of such beam-trusses. The beam theory (model) used for the calculations was easily able to generate magnitudes of the shear forces and these were usually reflected with precise (and extremely optimistic) faith in the sizes of the members making up the bridges—often virtually every diagonal of a lattice truss bridge was of a different section (Fig. 13.17). So entrenched were these attitudes that the first published analysis of the forces acting in the members of the statically determinate Warren truss was carried out using the beam analogy (model)—a far cry from nowadays when it is universally

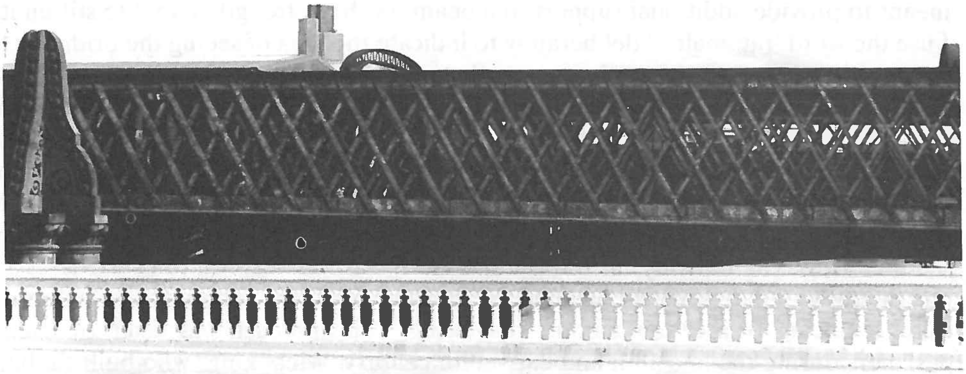


Fig. 13.17—Lattice truss modelled as a beam during design. Blackfriars railway bridge, 1864. Engineer: Joseph Cubitt

used to teach analysis by means of considering the equilibrium of joints [Doyne & Blood 1851].

In both the present and the previous section, the reader will have noticed the use of hyphenated terms to try to describe the types of bridge being built, arch-trusses and beam-trusses, and may recall the earlier reference to the ambiguous figure and the 'duck-rabbit' in §7.2. Such combined concepts really are needed to express the ambiguity with which they can be seen, now as a truss, now as an arch. This sort of ambiguity is characteristic of the state of uncertainty which separates two periods of 'normal' (in Kuhn's sense) structural design and represents the transition between two incompatible ways of looking at the problem.

Arch-braced trusses

The same duality between two ways of looking at how a structure works can be taken to apply to a second period of arch-trusses which grew out of the beam-truss period. Having created parallel chord trusses and found them to lack stiffness when used for long spans, the obvious idea presented itself to brace (stiffen) the truss by means of an arch. The result was a bridge which now was basically a truss bridge, with an arch added—the very opposite of the earlier arches stiffened by truss-bracing (Fig. 13.18).

Such structures could not be treated fundamentally as arches and so the late-19th century approach to justifying such designs, simply by means of arch theory borrowed from masonry bridge design procedures, could not be used. The approach which was developed was to estimate the proportion of the entire load which would be carried by the arch and the truss and then to assume that each structure acted independently [Haupt 1853]. (Amongst all these developments to truss bridge design, one should not, of course, forget the enormous contribution made by the increasing wealth of knowledge from precedent and controlled tests, to justify the new 'improved' designs, as they were often called.)

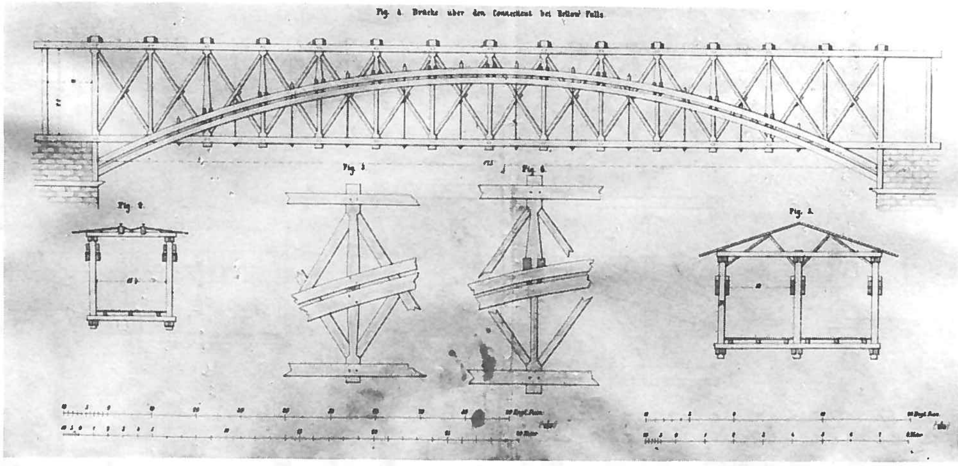


Fig. 13.18— 175-foot arch-braced truss over the river Connecticut at Bellow Falls [Culman 1851]

Statically determinate trusses

As a result of the considerable practical experience gained during the 1840s due to the enormous growth of the railways, especially in Britain, Germany and the USA, there was a general trend to reduce the numbers of structural members and to simplify the designs. This tendency was accelerated by the increasing use of wrought iron and the favouring of designs in which more and more of the members acted in tension. Such simplicity and the need to save on this costly new material, encouraged a consideration of the forces carried by single members. There thus arose the new possibility of making use of mathematical models involving simple statics and the triangle of forces, which had since the late 18th century become incorporated in very elementary ways in the design procedures to justify designs of roof trusses.† Since such methods were applicable only to statically determinate structures, this provided an immediate and additional drive towards designing simple structures, as similar as possible to the mathematical models used in their justification (Figs 13.19 and 13.20).

The whole idea of the truss bridge as working in bending like a beam was quickly abandoned and, for the first time, it became possible clearly to see a truss as a structure in which all the members carried either compression or tension loads. Colonel Long was one of the first engineers to talk of members designed specifically to carry tension forces—he even called one of his designs based on this principle, a 'suspension bridge', a description which continued to be used for several decades (see Fig.13.21 and [Cresy 1865:1353].

The importance of the triangle in bracing a structure was already familiar to ship designers (Figs 7.1 and 13.22) and, in less explicit ways, by some roof carpenters. The idea of statical determinacy in pin-jointed frameworks had been fully developed by the mathematician Möbius in the mid-1830s [Timoshenko 1953:304ff]. However, it

† The equilibrium of the forces in the rafter, the horizontal tie of a roof truss and the reaction on the support had appeared as a practical illustration of the application of statics in most mechanics textbooks since the mid-18th century.

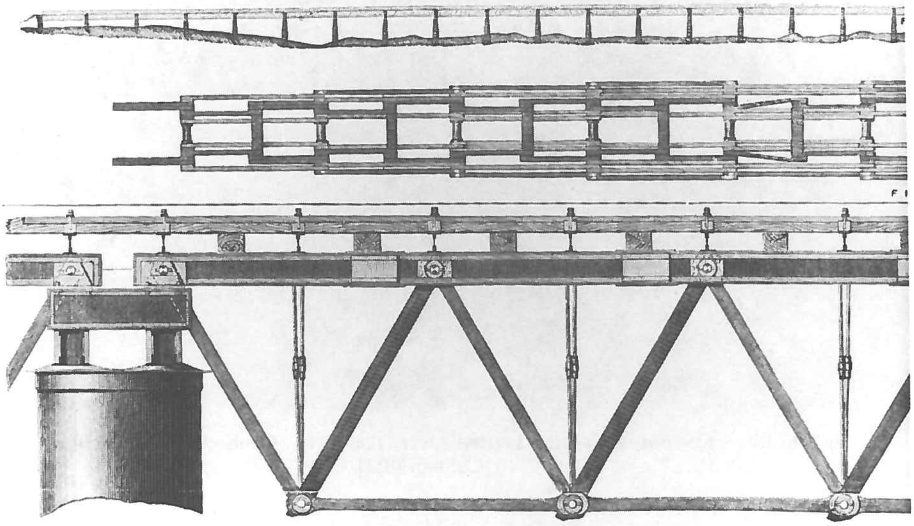


Fig. 13.19—A pin-jointed truss of the type popularized in Britain by Warren in the 1850s

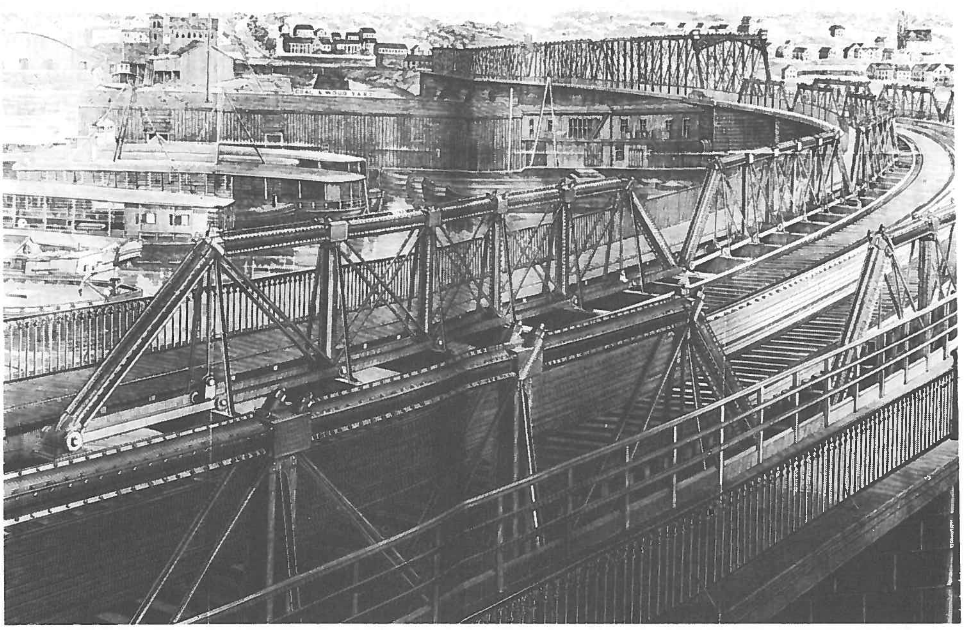


Fig. 13.20—An American truss combining pin-jointed eye-bars in the lower chord with rigid joints in the top chord. In design the truss would have been rendered statically determinate by treating it as fully pin-jointed and ignoring the counter-bracing. (Hudson River Bridge, Albany, NY [Maw & Dredge 1872]).

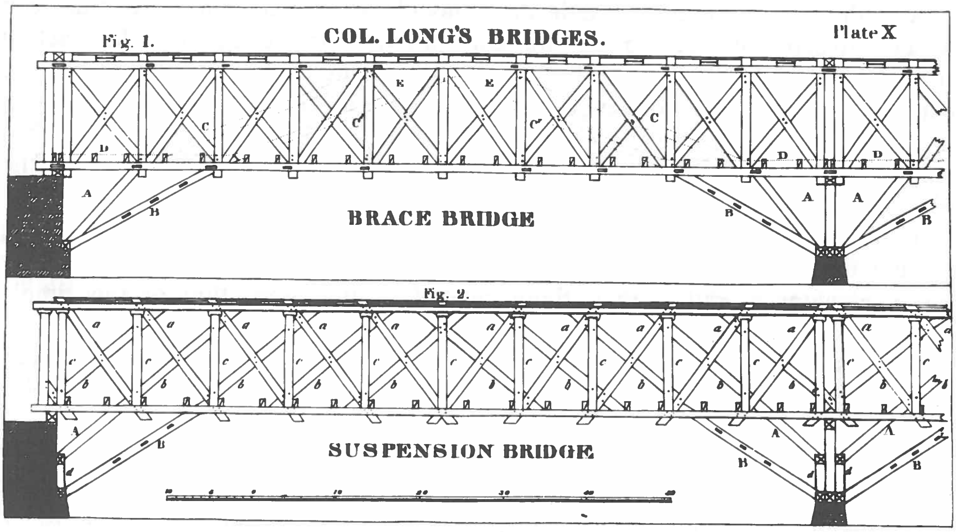


Fig. 13.21—Two truss bridge designs by Colonel Long clearly incorporating tension members [Long 1841]

was not until several decades later that engineering designers realized its importance in simultaneously providing bracing and rendering structures statically determinate [Bow 1851, Schwedler 1851]. This was probably because, up until the 1840s, truss bridges had rarely incorporated purely tensile members, since tensile joints in timber are difficult. This situation changed with the introduction of wrought iron into bridge construction, although this material had long been in use in roof trusses in which tension forces are unavoidable. Along with the introduction of wrought iron, and the 'possibility' of a statically determinate structure, there also came the possibility of a genuine pin-joint (impossible in timber). So it became both possible and desirable to construct pin-jointed structures.

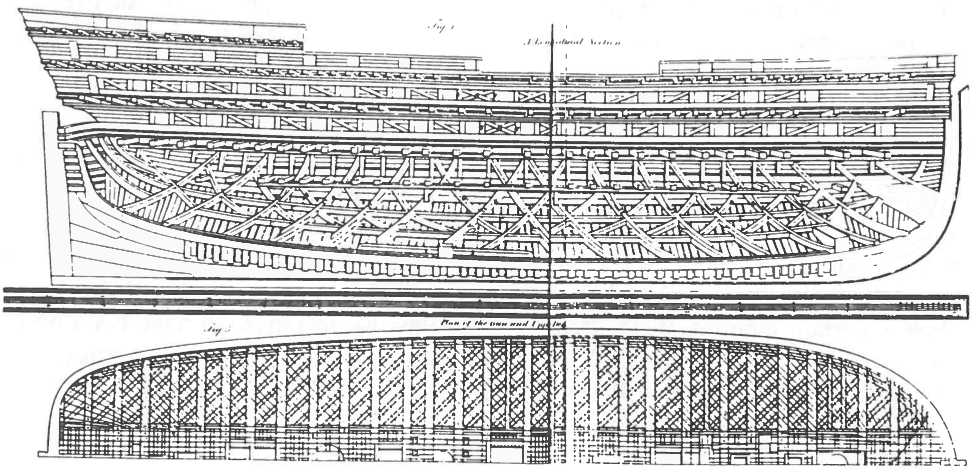


Fig. 13.22—The 'new principle' for constructing His Majesty's ships of war [Seppings 1814]

As with other developments in the art of modelling structures, the conceptual leap to see a structure in a certain way was concomitant with the idea of making actual structures which were real-world manifestations of the mathematical model in the expectation that this would cause the actual structure to reproduce the behaviour predicted by the mathematical model. This practice was often later shown to be a triumph of hope over experience when pin-joints siezed solid. Once the idea of the pin joint had been introduced, it was immediately seen to have other advantages, particularly concerning the standardization of components and the ease of transporting these components and assembling them in any part of the world. It was probably these advantages, rather than those related to the replication of the simple mathematical model, which fuelled the popularity of the pin-jointed truss.

As a result of this new way in which the truss could be seen as working, there also rapidly developed a number of techniques for calculating the results of complex systems of forces, especially in order to avoid the need for tedious arithmetic and trigonometric work. The long-known triangle of forces was resurrected and quickly transformed into a highly sophisticated analytical and geometric method for solving the mathematics of statical equilibrium [Schwedler 1851; Culmann 1851, 1852, 1866]. Such was the power of these techniques that it was also realized that structures which did not have pin joints could be modelled *as if they had* and the likely discrepancies between the model and the real structure could be taken into acount by the use of an appropriate factor of safety. This engineers came to 'see' pin-joints even when they did not exist in reality and, what is more, see them in types of structure which had existed before the introduction of actual pin-joints.

In its day, graphical statics must have had an effect on the life of the design engineer no less dramatic than the growth of computing and the availability of the electronic calculator in the 1970s, especially after the development of the elegantly simple 'reciprocal diagram' by Maxwell, Cremona and Robert Bow in the 1860s, and of the related geometric method of establishing the deflexion of statically determinate trusses by the Frenchman, Williot in 1877 [Timoshenko 1953]. A page from Robert Bow's book in which he introduces his well-known 'Bow's notation' illustrates the immediacy with which graphical statics could communicate a feel for how the structure was working (Fig. 13.23) [Bow 1873]. All of these developments helped to establish the enormous popularity of the pin-jointed truss during the third quarter of the 19th century.

Statically indeterminate trusses

The limitations of the statically determinate truss were felt very strongly. Many argued that it was not sensible to have structures with so little safety—the failure of any one member or joint would result in collapse. Construction engineers had long had a predilection for a certain amount of redundancy and some had already been exploiting one of the main advantages of redundant structures—their capacity to allow a certain amount of prestressing. It seems, for instance, to have been quite common in the USA in the 1840s to reduce the flexibility of timber truss bridges by driving in wedges to stress both braces of the St Andrew's cross bracing which was so popular [Long 1841, Haupt 1853]. However, the design procedures at the time had taken very little account of the stiffness of the materials—bridges were almost always designed according to ultimate load criteria, not deflexion. The overall deflexions of

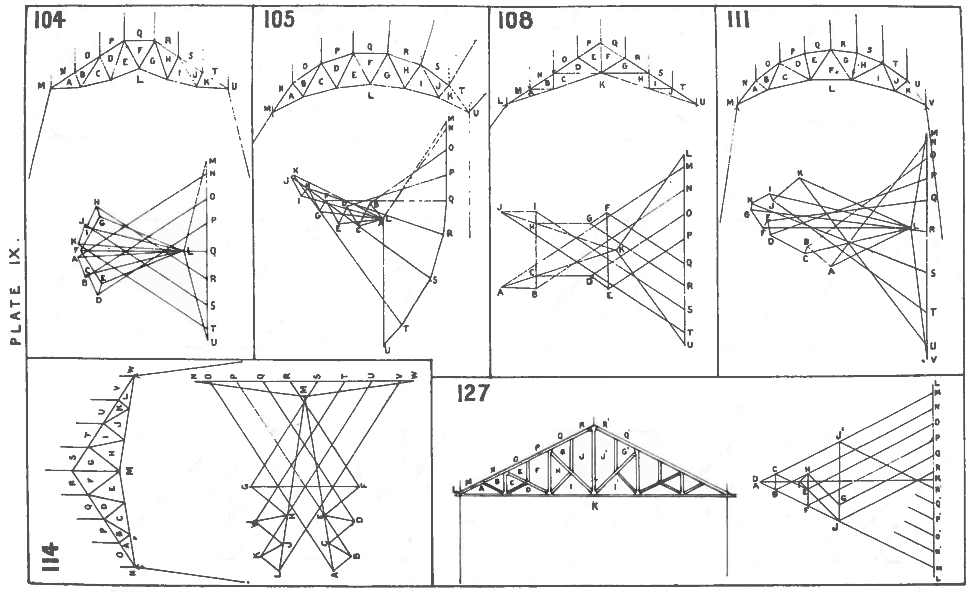


Fig. 13.23—Graphical statics as applied by Robert Bow to various roof trusses displaying instantly the effects of symmetric and asymmetric (#111) dead loading and wind loading (#105) [Bow 1873]

trusses were calculated only for very special cases when precedent and empirical rules based on the beam model of the truss were of no help.

The pin-jointed model, initially used only in design procedures for pin-jointed statically determinate trusses, spread widely and was soon being used for other trusses which were neither pin-jointed nor statically determinate. Redundant structures such as trusses with cross bracing could be rendered analysable in either of two ways. One was simply to ignore sufficient members to make it statically determinate, or to make the assumption (sometimes with good justification) that certain members would only carry tension, or compression forces—Jourawski and Whipple were two of the first to take this approach in the mid-1840s [Timoshenko 1953:181ff]. The other approach was to follow the precedent set by the design procedures developed for the arch-braced trusses mentioned earlier; the redundant structure was divided, notionally, into two or more separate statically determinate ones (Fig. 13.24). The share of the total load taken by each was estimated and each structure justified separately. The results were then combined by a rather unrealistic application of the principle of superposition. But, so simple were the design procedures based on graphical statics, that it was preferable to take account of the inaccuracies resulting from the over-simplicity of the pin-jointed model by adjusting the results using empirically established constants and factors of safety. This illustrates well how a revolution in the thinking applied to a particular case (here the determinate pin-jointed truss) enabled people to use that same mode of thinking to look at an old problem through new eyes and make new breakthroughs which could not otherwise have been conceived (as was also the case with the application of plastic theory to Gothic cathedrals, mentioned in §11.6).

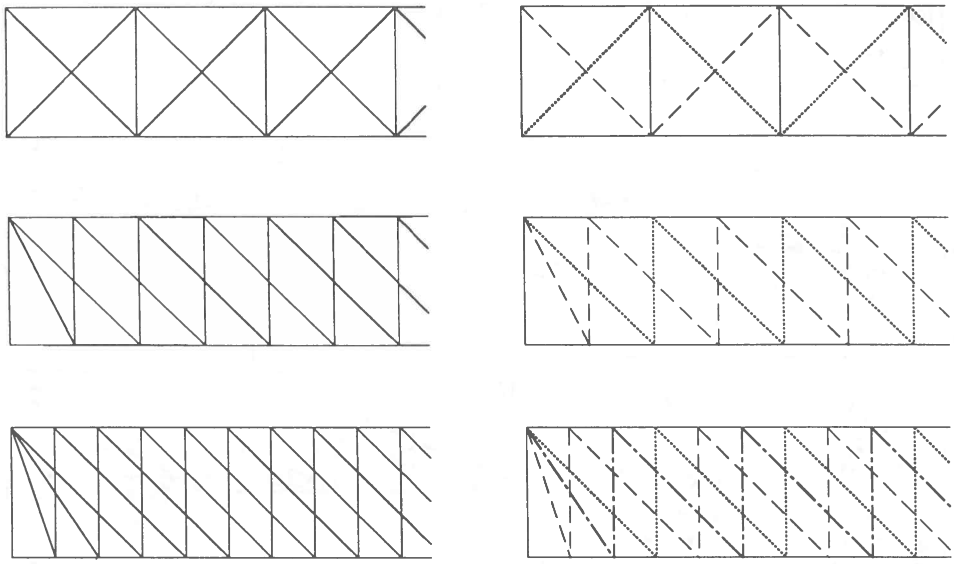


Fig. 13.24—Statically indeterminate trusses seen as two or more superimposed statically determinate structures.

It was in 1862 that a method for analysing the forces in the members of a redundant truss was first published (by Clebsch) but this, and others by Maxwell and Mohr (1874) and Castigliano's strain energy method (1879), were mathematically far too complex to be worth using in all except the simplest of structures [Timoshenko 1953]. The effect of this problem was a healthy one, since it continued to emphasize to design engineers that certain types of structure were preferable to others simply because they could be justified with relative ease and corresponding confidence. This principle is just as relevant today, although it has tended to be obscured by the computer's capacity to perform the complex and tedious mathematics with little effort on the part of the designer. Over the last quarter of the 19th century one can see an increasing preference for simpler stuctures and the avoidance of unnecessary redundancies such as cross-bracing and rigid supports. The criticism that statically determinate structures were more prone to catastrophic collapse in the event of a single member failing was met in another way—by the improved quality of materials and workmanship.

Rigid-jointed trusses

Despite the elegance of the pin-jointed model of trusses, many design engineers had a great reluctance to build actual pin-joints into their bridges. They preferred joints made using several ordinary rivets, sometimes reinforced by the addition of gusset plates, despite the fact that this would induce some bending into the structural members (Fig. 13.25). The deviation from the model used in the design procedure was consciously taken into account in the safety factors which were always employed and this was backed up by a great deal of testing, especially on the strength of rivetted joints. It was also realized by many engineers that the wrought iron or steel could suffer local yield in highly stressed parts (e.g. rivet holes) without threatening the

Fig. 13.25—A rigid-jointed truss

safety of a whole bridge (a qualitative anticipation of 20th-century plastic design procedures) [Unwin 1911/12]. And so, by the mid-1860s, most truss bridges were already usually being constructed with rigid joints, especially on the mainland of Europe, despite the fact that it was not possible to make any quantitative assessment of the effects of rigid joints in justifying a design [Mehrtens 1900] (the same change can be seen in roof structures during the early 1860s [Wittek 1964]).

Nevertheless, there was a growing feeling that, if the elastic design procedures being used for the design of trusses were to be exploited to their best effect, the stresses induced in the slender members of a truss due to the rigidity of the joints should be taken into account. It had also been found that, even in so-called pin-jointed structures, the pin-joints seldom remained free to rotate for very long, owing to both friction and corrosion. There was growing a feeling that the approximation to a pin-joint was becoming less and less believable and gradually the power of justification embodied in the simple elastic design procedures came increasingly under threat. There was a period of crisis, similar to that already discussed concerning steel-frame design in the 1930s and 1940s.

The minor design revolution which followed was to acknowledge the importance of the so-called 'secondary stresses' and to develop ways of in which the design engineers could calculate them—ways which would involve taking into account the the elastic properties (stiffness) of the material of which the members were made. After the initial rigorous theoretical and laboratory investigations made by Manderla during the 1870s, simplified approaches were developed and soon incorporated widely into bridge design procedures [Timoshenko 1953]. The design engineer's belief in the power of the design procedures to justify designs had again been restored. Trusses of this type—statically determinate (assuming pin joints) but with rivetted or other rigid joints, have been the norm since the 1880s for railway bridges (and timber roof trusses).

The portal frame 'truss'

A curious development of trusses, around the turn of the present century, was, in a sense, a direct progression from the previous one. As if to avoid the 'complication' of a twin structural model (involving both axial and bending stresses), and following the practice of the rapidly developing steel frame construction methods, the Vierendeel truss was conceived. This form is really not a truss at all, since none of the members are loaded axially and it is not triangulated. It works by portal action with all the structural members loaded in bending—the 'secondary' stresses have effectively become 'primary'. This type of bridge was able to make use of parts of the design procedures being developed for the frame structures of single- and multi-storey buildings. It did not spread widely outside Vierendeel's own Belgium, largely because of the complexity of the calculations involved in justifying a proposed design and because it offered little that other structures could not offer. There were also a number of collapses, probably due to the brittle fracture of steel at low temperatures. The Vierendeel form had in fact already been used some 50 years earlier in the cast iron girders of the roof of Durham station (Fig. 13.26). The idea has surfaced again recently as the 'Kubik truss' in the roof of an aircraft hangar at the new Stansted Airport. Being a space-frame version of the Vierendeel principle, it is hardly surprising it has not been used before—the calculations required to justify it would have been virtually impossible without the aid of a very powerful computer.

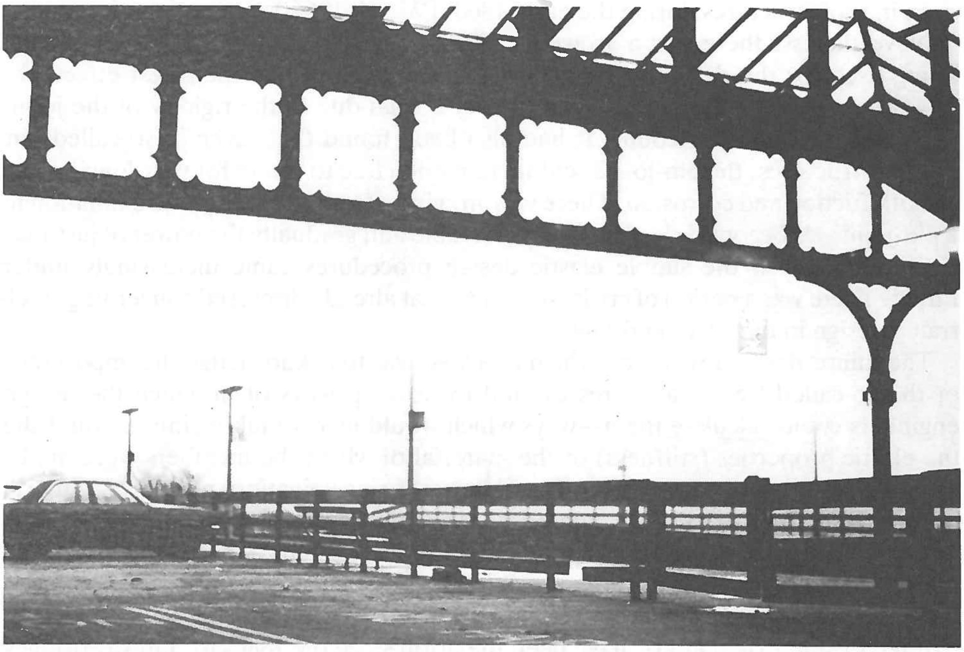


Fig. 13.26—The cast-iron 'Vierendeel truss' in the roof of Durham railway station (1840s)

13.4 SUSPENSION BRIDGE DESIGN

The history of suspension bridges has been rather dominated by the spectacular collapses of two bridges—those of the Brighton Chain Pier in the 1830s and of the Tacoma Narrows bridge in 1940. Dramatic pictures of the collapses often find their way into histories of engineering, not only to give a sense of the forces with which engineers must contend, but also as illustration of the mechanism by which engineering is commonly supposed to progress, namely, 'trial and error'.† However, in terms of their influence on subsequent history, the two cases could not have been more different.

The Chain Pier at Brighton comprised four spans of 255 feet and was opened in 1823. Captain Samuel Brown, its 'scientific projector and builder' had already completed several similar bridges using his patented chain link construction, including the 449 ft Union Bridge which was opened in 1820, and severely damaged by wind in the following year. In October 1833 Brighton suffered a violent storm which destroyed three of the pier's spans leaving 'twisted rods, broken and splintered woodwork and shattered towers'. After several donations and a house-to-house collection, £1300 was raised and 'the chain pier, by sundry judicious alterations, was made stronger than ever'. Little more than three years had elapsed, however, when the pier was struck by 'a terrific hurricane'. The third span of the pier was again destroyed:

> About half-past twelve in the day the centre bridge seemed to have acquired, through the force of the wind, a vibratory motion, which soon after more or less affected the whole structure. At times the platform was raised to the level of the protecting iron rails at the sides of the pier. Eventually one of the towers began to rock, and the piles also to twist; and finally the platform of the third bridge was lifted up from its bed several feet, and, falling again,—the suspension rods being unable to bear the stupendous strain,—plunged into the stormy waters below. [See Fig. 13.27] Almost from that moment, strange to relate, the storm abated. [Bishop 1897:33]

Again the pier was restored, this time including ties from the underside of the deck, roughly at quarter span, to the tower piles. It survived another 60 years but towards the end its condition had deteriorated to such an extent that it was closed to the public because one of the towers was 'no less than 6 ft. 9 in. out of the perpendicular'. It finally fell in a storm on the night of 4th December 1896:

> Though the fate of the Pier was expected, there were very few eye-witnesses of the final scene. One of them was Miss Body, who lives in one of the houses under the cliff, through which the suspension chains pass. About half-past ten o'clock Miss Body was startled at a tremulous movement of the chains, which shook the house in a manner that they had never done before, and, in some alarm, she called out, 'What is the matter with the pier?' and, going to the window, was just in time to see the pier collapse and the giant chains sink to the ground. [Bishop 1897:51]

The lively contemporary accounts of these collapses and others, such as the Wheeling Suspension Bridge in 1854 [Petroski 1985:167ff], are many people's idea of, what history is made of but in no way do they reflect the significance of such events

† For the design of early suspension bridges and the considerable progress made without the 'need' for failures, see [Peters 1987].

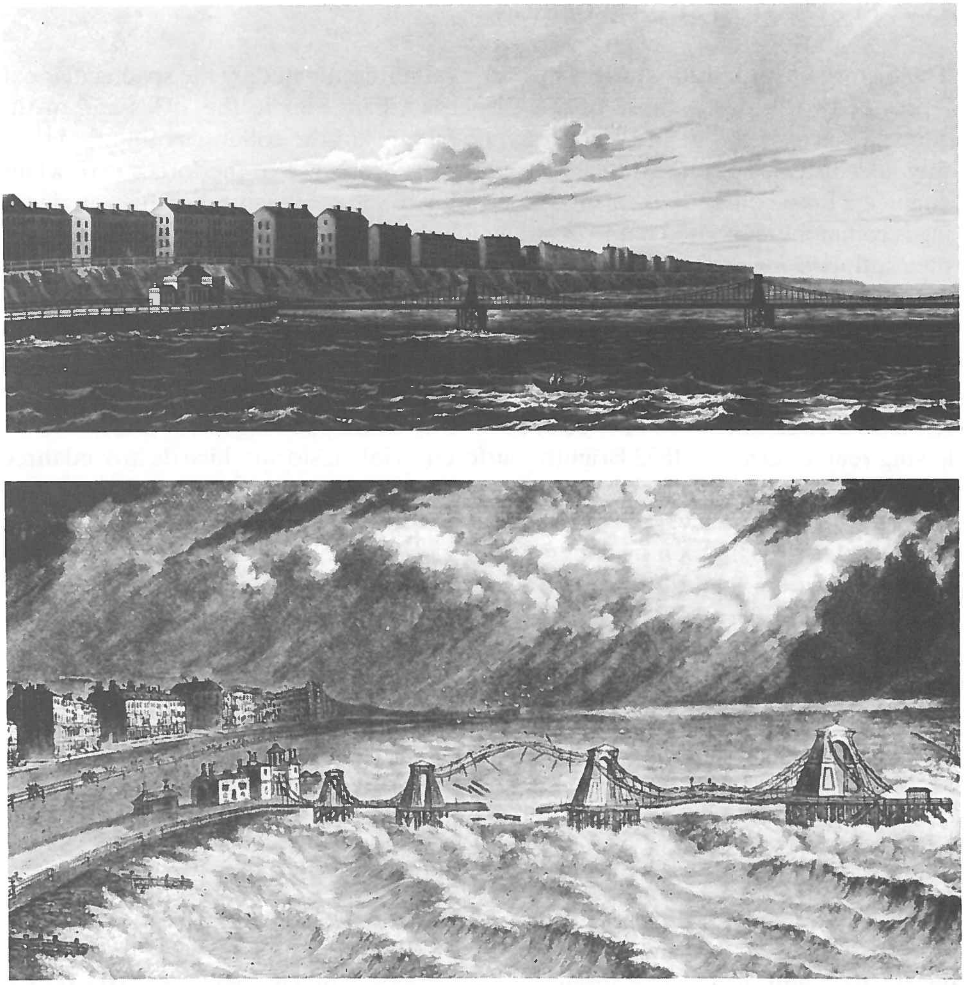


Fig. 13.27 Brighton Chain Pier: (a) at its opening in 1823; (b) in the hurricane of 29th November 1836 [Bishop 1897]

in the development of the design of suspension bridges. In fact, in the case of both Brighton and Wheeling, the effect was very little, except to alert contemporaries to the dangers inherent in suspension bridges. No attempts were made to change the general approach to their design by investigating the dynamic behaviour of such bridges.

It must remain a matter of debate as to why wind and dynamic phenomena were not taken more seriously during this period, despite evidence of their destructive power. Apart from the question as to precisely what anyone could have done about it, economics were probably a major influence—the chains or cables of suspension bridges were at the limits of technical possibility and the additional costs of research and the stiffening of bridge decks would probably have made them uncompetitive. As it was, the reputation gathered by suspension bridges did have its effect in Britain where five suspension bridges were damaged or destroyed by wind between 1818 and

1839, including substantial damage to Telford's bridge over the Menai Straits in 1839. A hundred years later the report of the inquiry into the collapse of the Tacoma Narrows bridge noted that it was 'not surprising' that few large suspension bridges had been constructed in Britain since that period [Farquharson 1949:13].

Despite a further five suspension bridges being severely damaged by dynamic wind loading in other parts of the world, progress in suspension bridge design during the 19th century was not much influenced by these incidents. Some bridges which did move too much for comfort were given the same remedial treatment which the Brighton Chain Pier received at some time after the 1836 storm - the provision of stays between the tower base and the underside of the deck in the region of the quarter point of the main span. Roebling took the decision to add such stays to his Niagara River bridge while it was under construction when he heard of the Wheeling bridge collapse, although he had no reason to believe that his bridge might suffer the same fate.† Engineers continued well into the present century to view the design of suspension bridges as a matter of resisting only vertical dead and live loads and a constant lateral load due to wind—continued, that is, until the collapse of the Tacoma Narrows bridge near Seattle in 1940.

As with most accidents and collapses, the reasons why they occur turn out to be a highly particular cocktail of circumstances and events, of design details and construction methods, none of which were new in themselves, and, of course, of cost pressures and last-minute changes of mind. The problem with the Tacoma Narrows bridge was clear for all to see as soon as it was completed—in relatively light winds the deck of both the side spans and the main span of the bridge oscillated in a twisting fashion which gave immediate cause for concern. Three months after its opening on 1st July 1940 stays were fitted to restrain the side spans in the hope this would adequately stiffen the main span. They did not and the result was some of the most spectacular feet of film ever shot when the bridge collapsed on 7th November in a wind of 37 knots while twisting at a frequency of 36 cycles per minute. The whole engineering design community was thrown into a state of crisis—a previously reliable way of designing bridges had failed for an unknown reason (although, as we shall see, some concern about the effects of wind had already been raised and ignored). The existing design procedures could no longer be relied upon to give an adequate justification of a proposed design for a bridge. The story of the investigations following the collapse of the bridge give a rare insight into how a crisis can change the way a structure and its behaviour appear to engineers, and result in an entirely new approach to design—a design revolution.

The bridge collapsed as a result of torsional oscillations of the deck—a mode of failure which had not been considered by the designers. As was usual at the time, the deck had been designed principally to resist vertical loads and a static lateral load due to the wind (30 lb/ft^2 acting on 1.5 times the exposed area). The cross-section of the deck was like a very narrow I-beam (on its side), the flanges ('stiffening girders') of which were 8 ft deep and 39 ft apart. The reinforced concrete roadway was supported on longitudinal stringers spanning between transverse floorbeams 4 ft 4 in deep. Within the depth of the floorbeams the deck was braced laterally to resist static wind loads. The main span of the bridge was 2800 feet.

† Since it was one of the very few 19th-century suspension bridges designed to carry a railway, its deck needed to be so stiff that it was highly unlikely to suffer from serious wind-induced oscillations.

It is difficult, with hindsight, to comprehend how people failed to see how flimsy such a ribbon of steel and concrete would be, especially in torsion, since an I-section has very poor resistance to torsion. Yet just this happened. Contemporary design procedures contained no specific reference to the torsional stiffness of bridge decks. Any torsion caused by asymmetric traffic loading on the carriageway was seen as a small and very local change to the vertical loading on the main suspension cables—a special case of a point load. This would be resisted by the mass of the whole cable/deck system, by the 8 ft deep stiffening girders and by the stiffness which the deck and stiffening girders *happened to have* by virtue of their being joined together. The reason for what appears to have been an oversight was the additional fact that contemporary design procedures contained no mention of any type of loading which could give rise to torsion in the deck. It was, therefore, not unreasonable to ignore it.

Yet all these facts applied equally to other suspension bridges which had not suffered oscillation problems. The Tacoma Narrows bridge was different in several ways which turned out to be significant. It was unusually narrow and much less deep, compared to its span, than previous bridges. The sag of the main cables was unusually low. The deck was stiffened by girders, rather than trusses, which were more usual. After work on the bridge had started, the deck itself was redesigned in order to save on structural steel and reinforcement in the concrete roadway. All these factors contributed to the reduction of the in-built torsional stiffness of the deck. In previous bridges this in-built stiffness had just happened to be sufficient to provide adequate torsional stiffness. They had also had greater mass, more frictional damping and decks of different aerodynamic cross-section, all of which would have helped to prevent any oscillations building up.

It is of great significance that the cause of the bridge's problems which had not occurred to its designers was identified within two weeks of the collapse by Theodore von Kármán, an engineering scientist working in the field of aerodynamics. In a letter to the *Engineering News Record* he reported his own experience of fluttering aircraft wings and used this model to calculate the wind speed which could have caused this problem in the bridge. He was within 20% of the actual speed [Petroski 1985:166]. In principle, then, the bridge engineers could have taken such factors into account during design, at least to the extent that a previously unknown phenomenon was not involved (unlike the 'discovery' of metal fatigue in the 1840s). However, bridge engineers do not see their bridges through the eyes of scientists, but through the design procedures which are current among their profession at a particular time.

Immediately after the collapse of the bridge a number of research programmes were begun. One was to investigate the dynamic behaviour of suspension bridges. Another was to establish the origin of the forces which caused and sustained the oscillations. Finally, and for the designers of the future, most importantly, a means was sought by which the dynamic behaviour of a bridge could be predicted with a reliability which would satisfy engineers—by which a proposed design could again be justified satisfactorily. All the research programmes involved the use of physical models of the bridge.

It was already a well-established practice to use a static model during the pre-construction phases of building a suspension bridge. Its main function was to help to plan the sequence of erecting the many small sections of the bridge deck. The 1:100 model was two-dimensional and represented only the vertical loading on the bridge; it had

only one suspension cable and the towers were effectively completely rigid. Already during the construction of the bridge, its flexibility was noticed and it was realized that the performance of previous bridges would be of little help in predicting the final behaviour of the Tacoma Narrows bridge. Concern mounted when rumours were received that another suspension bridge with plate-girder deck stiffening was suffering from wind-induced oscillations (the 2300 ft Bronx–Whitestone bridge, opened in 1939). The decision was taken in late 1939 to construct a new 1:100 model of the Tacoma Narrows bridge giving 'full dynamic similitude'.

The idea of a three-dimensional dynamic model had already occurred to the engineers working on the two-dimensional model, but they had in mind only to investigate the effects of moving loads on the bridge and of the impact of gusts of wind—they had already recognized the susceptibility of the slender deck to sinusoidal oscillations, but in the horizontal plane, not vertically (even though no such oscillations had ever been reported in completed bridges). Loads simulating a gusty wind were applied electromagnetically at 100 places to the weights representing the bridge deck and it was quickly found that lateral oscillations presented no problem. At about the time the model was completed, rumours were heard that yet another bridge completed in 1939 was suffering wind-induced undulations and it had also now become apparent that the Tacoma Narrows bridge was 'a very lively structure'. These factors, and the experience already gained with the dynamic model all brought home to the investigators that their new model was still too crude to represent the relevant features of the structure. It was found that the damping provided by the bridge towers was crucial and the original towers of the model were replaced by ones which represented both the longitudinal and the torsional stiffness and allowed control of the damping they provided.

In parallel with the mechanical investigations, other research teams were looking for the origins of the periodic forces which the wind appeared to be exerting on the real bridge. A series of static wind tunnel tests were carried out on a model of the cross-section of the deck. Two theories were investigated—the possibility of wind gusts of a periodic nature induced by the terrain in which the bridge stood, and the possibility of the wind amplifying a twist caused by a chance gust acting on the upper or lower surfaces of the deck. This 'autorotation' theory had already been used to try to explain the vibrations of electricity transmission lines in wind. The periodic gusting theory, which probably had its roots among the engineers who had first wanted to build a dynamic model of the bridge, was soon discarded. The static wind tunnel tests quickly established that both the shape of the cross-section of a bridge deck and the angle of attack of the wind made an enormous difference to the forces acting on the structure (although the current design procedures made no mention of either of these). It was also found that the Tacoma Narrows bridge deck was particularly likely to lead to vertical forces which could initiate the motions established as a feature of the bridge which by now was nicknamed 'galloping Gertie'. However, it was also shown that the autorotation theory could not account for the *sustaining* of the bridge deck's torsional oscillations. At this stage there was no mention of vortex shedding.

Although the several investigations had helped to understand the reasons behind the bridge's problems and helped to evaluate the effectiveness of the hold-down stays applied to the side spans, they provided nothing which could help to save the bridge. They did, however, demonstrate that a radically new approach to understanding the

behaviour of suspension bridges was needed. It had already become clear *before the collapse* that the current understanding of the behaviour of suspension bridges was inadequate and that the engineering community as already in a state of (Kuhnian) crisis. It was realized that it would be necessary to gain much more experimental experience of the effect of wind on bridges before another major suspension bridge anywhere in the world could be contemplated. It is thus likely that the same ultimate progress in bridge design procedures would have been achieved even if the bridge had been rather stiffer and, like many others before it, had simply oscillated in an alarming manner, suffering perhaps only slight damage. The collapse itself was by this time almost inevitable. When it did occur it, in fact, added relatively little new information of help in the subsequent investigations, however, the spectacular nature of the collapse perhaps helped to secure the funds needed for the subsequent research.

Within a few months of the collapse two 1:200 models of the bridge were built, this time not only trying to represent the static and elastic behaviour of the towers, cables and deck, but also having geometric similitude. They were tested in wind tunnels to try to recreate the oscillating behaviour but both were found to be too small to give useful quantitative results and failed to represent the damping adequately. It was realized that a much larger model with even more accurate representation of the stiffness of the bridge would be needed. In addition, some experiments in the summer of 1941 using photo-elastic flow visualization techniques demonstrated the creation of vortices in the wake of the flow past the deck section.† Their importance was immediately recognized as a likely cause of a periodic vertical load on the bridge deck and confirmed the need for the model precisely to represent the bridge's geometry.

Thus, by late summer 1941, it was resolved to build a special wind tunnel at the University of Washington to test a 1:50 scale model of the bridge in laminar airflow at scale speeds of between 0.5 and 18 mph (the main span of the model was 56 ft long). It was also decided to use the tunnel to test the behaviour of the new bridge, designed between 1943 and 1945 to replace the one which had collapsed and opened in 1950. The outcome of the testing was highly successful. All the modes of oscillation observed in the original bridge were reproduced in the model. The generation of vortices and the damping of the whole moving system were found to be crucial. It was, however, also realized that expense would prohibit the testing of such large models as part of the design procedure for all future suspension bridges. Consequently, further investigations were undertaken using 'section models' mostly at 1:50 scale and about 5 ft long, to correlate the results with measurements made on the full model. By methodically varying the way the section model was supported, the experimenters were able to generate the results which would enable the dynamic behaviour of an entire bridge deck to be predicted from the behaviour of a model of only a short section.

The 10-year research programme following the collapse of the Tacoma Narrows bridge established three particular conclusions concerning the form and stiffness of

† These techniques, incidentally, had only been developed in the two or three years previously as part of the increasing interest in fluid flow by the aerodynamic engineering community involved with aircraft development.

the decks of suspension bridges and the benefit of introducing damping:

- truss-stiffening of decks was preferable to girder stiffening since it substantially increased the wind speed at which oscillations would occur;
- the torsional stiffness of a truss-stiffened deck was usefully improved by incorporating lateral bracing at both the top and bottom of the trusses (so forming the box section which was achieved literally, using welded steel plates, in the Severn Bridge some 25 years later);
- the overall tendency for the bridge to oscillate could be significantly reduced by connecting the deck and main cables longitudinally at centre span by ties incorporating damping action.

Of wider significance was the establishing of a radically new approach to designing suspension bridges which, for the first time, involved the use of wind tunnels. The report of the investigations concluded that the use of the section-model tests and the associated mathematical analysis would yield 'adequate knowledge of the aerodynamic characteristics of a proposed suspension bridge' [Farquharson 1954:94].

Finally, the story of the Tacoma Narrows bridge illustrates two phenomena which it shares with many other design revolutions. Until the year of the collapse, the early history of suspension bridges was virtually unknown to most design engineers. Even a slight knowledge would have given ample evidence that wind had been a problem in earlier times. The problem of vibrations resulting from small periodic loading was also known—many smaller suspension bridges had once carried notices instructing marching troops to break step on the bridge: with the increasing size of bridges (and perhaps fewer troop movements) this problem had passed into obscurity.† Secondly, the inquiry into the collapse exposed the blindness of the contemporary engineering community to important evidence of the problem which later manifested itself in a catastrophic way at Tacoma Narrows. The problem of wind had, in fact, already been noticed and rumours were already rife by early 1940. The inquiry found evidence that no fewer than five major bridges constucted in the USA during the period 1937–39 (including the Golden Gate bridge) had suffered from disturbing oscillations of several feet amplitude. Yet, so far were the effects of wind from people's minds, that engineers did not recognize the significance of the evidence and it was ignored, or, at best, believed to be the result of unique circumstances and of little concern. All this provides a salutary tale of the consequences of being ignorant of history and of engineers becoming too deeply and blindly set in their attitudes to design.

† By what seems to have been a coincidence, the year following the collapse saw the publication of an excellent bibliography on the history of suspension bridges with over 5000 references [Jakkula 1941]. In the preface, the author mentions devoting over ten years of his spare time on the work and makes no mention of the publication being influenced by the Tacoma Narrows collapse.

14

Revolutions in engineering design

In the previous three chapters we have looked at a number of examples of radical changes to the way various types of structure have been designed in the past. By implication, during the periods between the major changes, there have been periods of relative stability with only minor changes occurring. This pattern reflects the two types of development which Kuhn has identified in the historical development of science—*scientific revolutions* and *normal science*. With this parallel in mind I have referred to the radical changes in the history of engineering design as 'design revolutions' and borrowed other concepts from Kuhn, such as *crisis* and *paradigm*, to help to reveal the nature of the events described.† The main difference has been to replace Kuhn's attention to the role of the *scientific hypothesis* in the history of science by a concern for the *design procedure* in the history of engineering design. Just as Kuhn suggested that 'theories are, even more than laboratory instruments, the essential tools of the scientist's trade' (see §9.4), so:

> design procedures are, even more than scientific theories and mathematical techniques of structural analysis, the essential tools of the engineering designer's trade.

The design procedure can thus be seen as an appropriate 'parameter' in terms of which to study structural engineering design history.

In proposing a Kuhnian view of the history of engineering design, I am not, of course, suggesting that there is anything incorrect about historical events which has already been recorded. Rather, I am suggesting an alternative view or interpretation of those events—a view which resolves many of the problems about the history of

† This is not the first time that Kuhn's ideas have been used in the history of engineering context. In brief papers, two authors have made cursory use of Kuhn's idea of the paradigm: the first in expressing his general view of design in civil and structural engineering [Brown 1970]; the second in advocating a new approach to pedagogy in structural engineering [Brohn 1982]. Two other authors have also made use of Kuhn's ideas to discuss historical development in other branches of engineering, concentrating particularly on the role of inventions. One author refers to Kuhn in a general way, in treating the history of mechanical engineering [Duffy 1979]: the other has made more extensive use of the idea of revolutions in the context of the invention of the jet engine [Constant 1973, 1976, 1980]).

engineering which were mentioned in Chapter 2. In particular, the traditional histories tend or render the history of design invisible and a number of key questions are not tackled head-on:

- how did engineers (or masterbuilders or architects) design their works?
- before the existence of modern theory, what did engineers use in its stead?
- when engineers made use of theory, how did they use it?
- when they did not, why not?
- what use was, and is, made of other bodies of knowledge in addition to theory?

A number of more general issues, concerning progress and the historical development of engineering, were also identified:

- civil and structural engineering design has not followed a single line of historical development leading directly to the present state of the art;
- progress in engineering design has not been led by theory; in fact, developments in theory have usually resulted from the demands of engineering designers;
- progress nowadays is no less a matter by learning from mistakes ('trial and error') than it was in the past;
- at any one time, there have often been several viable ways of designing a certain structure which may have been incompatible with one another;
- there is no date before which design was empirical and after which it was 'scientific';
- successful design, both in the past and in the present, can be undertaken using 'incorrect' theories now discarded by engineering scientists;
- there is a danger of inadvertently transporting into the past our modern concepts and attitudes concerning design.

The way in which a Kuhnian approach to history may help to deal with these issues can now be discussed in a general outline of the structure of engineering design revolutions which elaborates the details of the previous chapters.

14.1 RECOGNIZING DESIGN REVOLUTIONS

It is undoubtedly a valid question to ask why, if revolutions in the history of science and design are so significant, have they not been noticed until Thomas Kuhn saw (or invented?) them. In the case of science, Kuhn discusses this 'invisibility of revolutions' at length (see §9.5 and [Kuhn 1970a:chap.11]). To some extent it is a case of history being written by the victors - the main way in which science is passed on to a new scientific community is by means of education, in the form of textbooks and popularizations of science in periodicals and the media. Since authors have every reason to want to make their work appear as up-to-date, clear, convincing and self-contained as possible, they will not want to confuse issues by pointing out that there have been, or still are alternative views on the subject. However, this aim tends to grow to include rewriting the historical development of the subject almost by way of trying to give the new beliefs greater credibility by appealing to pedigree. We have seen how this was the case with the domination of the elastic theories of structural

behaviour which virtually excluded all mention of previous work on materials and structures beyond their elastic limit (see §11.6). A further difficulty lies in the very existence of a current body of historical data. These have been collected and presented by historians who were not looking for design revolutions and, indeed, did not find them. They have interpreted historical 'facts' to suit their own models of history. In presenting a new view of history, original sources need to be re-read with new eyes.

Another important aspect of the adoption of new paradigms after a revolution is the loss of understanding of the old paradigms. With the use of new concepts and new vocabulary, for example, we come to see and think and talk about the world in a new way; and this includes our perception of the past as much as the present. We cannot now even conceive how a cathedral designer in the Gothic period perceived his building. This makes the interpreting of both current records and the structures themselves extremely difficult and liable to gross and unconscious misinterpretation.

In addition to suffering the same tendency towards invisibility as scientific revolutions, design revolutions have additional problems which make them difficult to see. The revolutions being studied are intellectual ones in a field which is normally a highly practical activity. They are seldom accompanied by dramatic, publicly observable and recordable events, as are many political revolutions and that popular milestone of historical development in technology, the invention. Even the direct evidence of the completed artefact seldom reveals the manner in which it was designed: consider, for instance, the cases of a Gothic cathedral and a modern steel-frame building.

A second difficulty arises in the paucity of useful records—the engineers' 'papyrophobia'. Engineering design procedures have seldom been written down in the past and, even in the present, they are only to be found by inference from the Codes of Practice and the calculations which a designer produces to justify his design. Both of these sources tell only a small part of the story, most of which goes on in a designer's mind, on scraps of paper which are thrown away and in design meetings. There is also the difficulty, and pointlessness, of writing down how to do something which you know how to do, and which is generally learned during the course of an apprenticeship and, later, in exercising the skill professionally. Indeed, there are also strong disincentives (business common sense) to publishing trade secrets, be they those of the mediaeval mason or the computer programmer.

Notwithstanding all these difficulties, evidence of design revolutions can be found when we know what we are looking for. They can be recognized by a number of symptoms which are characteristic of revolutions:

- the existence of separate *design communities* and their own *paradigm* design procedures;
- the appearance of *anomalies* in the results of using established design procedures, some of which are easily resolved and some of which lead to periods of *crisis*;
- sudden changes of direction or discontinuities in the historical development of structural designs or the appearance of entirely new design procedures;
- heated debate between different groups of professionals about the relative merits of alternative design procedures;
- the appearance of new use of words and concepts in the language of engineering design.

Before proceeding to discuss these various symptoms of revolutions in the remainder of this chapter, a further aid to recognizing design revolutions should be mentioned. We have seen that several (but by no means all) design revolutions during the last two centuries have been closely associated with revolutions in engineering science. Indeed the latter have often been prompted and driven by the needs of engineering designers who have found themselves in a state of (Kuhnian) crisis—the plastic design revolution is a case in point.

14.2 DESIGN COMMUNITIES

Design communities can exist at different levels. A whole age, for instance, can be characterized by a single type of design procedure:

- the use by the Greeks of particular ratios and proportions based upon the human body, harmonics and simple geometrical figures;
- the use, during the Gothic period, of procedures somehow based upon the Bible, Euclid, harmonics and precedent;
- the use of simple statics to determine the safe design of masonry arches in the 18th century;
- the use of elastic beam theory for the design of timber and iron truss bridges in the early 19th century;
- design procedures, current during the whole of the latter half of the last century, for most types of fabricated wrought iron and steel structures based on the pin-jointed model and the techniques of graphical statics;
- the universal use of elastic design procedures for all types of structure between about 1830 and 1950.

At a more specific level, distinct design communities may exist simultaneously, being distinguished perhaps by country, the particular field of operation, type of material used or level of sophistication. For instance:

- During most of the 19th century it was usual in Britain to design to a limiting stress, whereas French design procedures involved limiting strains (see §10.2.2).
- Codes of Practice for a particular building type may differ from country to country regarding their recommendations concerning types of loading, working stresses and deflexions, type of structural analysis, degree of safety, permissible grades of material, and so on.
- In the 1930s structural design procedures in the aircraft industry used load factor techniques, whereas in the construction industry, stress factors were the accepted way of incorporating a degree of safety into a design (see §11.5).
- During almost two centuries following Galileo's work, two different design procedures were used for beams made of flexible and non-brittle materials such as timber, and of rigid and brittle such as stone and, later, cast iron. They involved not only different theories of bending, but also very different factors of safety [Tredgold 1820, 1822; Timoshenko 1953].
- The design procedure for one type of structure might include the need to build

and test a model of the proposed structure, while for another type, it might not—identifying the requirement for model testing is a crucial part of the design procedure [Janney 1964; Smith 1977].

Occasionally the existence of separate (and imcompatible) views about design is mentioned in print. We have already met the irreconcilable debate between the advocates of plastic design and elastic design methods (see §11.6). Concerning the elastic design of structures, in a paper entitled 'The limitations and application of stuctural analysis', Hardy Cross discusses his classification of statically indeterminate structures into three groups according to the significance and sensitivity of the calculated stresses in different parts of a structure. He then goes on to identify two 'schools of thought':

> This sort of differentiation of structural type may serve to clarify, if not to reconcile, two conflicting viewpoints that have always run through the literature of structural analysis in America. One group of thinkers, of the older school, claims that the indeterminate structure is always inefficient, that the different parts always interfere with each other. The other holds that indeterminacy always has special virtues, of which economy is one. The older school seems to have erred in grouping all indeterminacy under the inefficient group indicated in the third heading [of Cross's classification—the failure of other members to shed load when one member carries more load], but the other school errs equally in neglecting [this] important classification [the distinction between efficient and inefficient types of indeterminate structure] entirely. [Cross 1935b:108]

In another example, different design communities have been identified implicitly in a rare type of paper, one devoted to the philosophy of design. Under the heading 'Evolution of design attitudes towards stability', the author discusses several different approaches taken by designers according to the precise design context and type of structure. In particular he mentions two main categories of design procedure:

> Structural engineering designers have tended to approach stability problems in two extreme ways:
> (1) The checking of stability mechanisms to ensure structures are 'rigid'. It is clearly important to ensure any structure is not a near-mechanism, and the avoidance of near-mechanisms has always been a feature of good structural design.
> (2) Checking that buckling deformations are within acceptable bounds. This usually involves an assessment of critical loads, and ensuring such critical loads are not encountered in practice. [Chilver 1976]

Finally, and at the most detailed level, even in identical circumstances (location, time, sector of industry, type of project, material etc.), there are differences in the design procedures used by different companies. Each company has a kind of 'house style' which it has developed through many years of experience and manifests itself as a large number of preferred procedures for designing both whole structures and their many details.

At each of these three levels, comparable design procedures can differ both in their manner of describing and of justifying a proposed design. However, it is particularly in the means of justification that the identity of design communities is most likely to be exposed. By its very nature the justification needs to provide reasons why a particular design is preferred, and it is in the very nature of what can constitute a reason that design communities might differ most passionately.

14.3 ANOMALY

Kuhn uses the word 'anomaly' to describe the discrepancy between a 'paradigm-induced expectation' and what is observed, for instance the position of a planet predicted using Ptolemy's model of the universe, and where it is observed to be. In the context of engineering design, the situation is rather less clear to see. Take, for example, the case of using simple beam theory to predict the behaviour of a steel beam. If carried out by an engineering scientist the deflexion under load could be measured directly, or the stress in one of the flanges could be inferred by measuring the surface strain using strain gauges and interpreting this as a corresponding stress using a theory of bending. If the measured deflexion or surface strain turned out to be significantly less than or greater than the value predicted using the currently accepted paradigm theories, this would constitute an anomaly and subsequent investigation would try to ascertain an explanation. If, however, an engineering designer used the same paradigm theories to predict likely deflexions and maximum stresses in the steel, he would do this without the slightest intention of returning to check the actual deflexions or stresses. Furthermore, the theory would have been used in conjunction with one or more empirical constants such as a factor of safety. A large overestimate of the predicted deflexion would never manifest itself unless the resulting beam was believed, for other reasons (such as experience), to be stiffer than necessary. Alternatively, if the prediction turned out to be an underestimate, there are a great many reasons which might lie behind the anomaly—faulty workmanship, accidental overload, an inadequate model of the whole structure of which this particular beam is a part, a poorly conceived design which behaves in ways difficult to understand, and so on. Other types of failure may have contributed to the problem, such as poor resistance to fire or corrosion. In other words, the validity of the theory of bending is only one of many factors which may have contributed to the discrepancy between predicted and observed behaviour.

Apart from the most obvious type of anomaly, a failure due to excessive deflexion or collapse, for example, there is a second type which is more difficult to detect since it arises without a serious collapse having taken place. It might arise when a designer somehow comes to doubt the ability of a familiar design procedure to provide a justification of a certain proposed design—any designer will do this as a matter of course when considering a new structure and might choose to adapt an old procedure or create a new one to suit. This is clearly necessary when there is no precedent to follow. It may be, however, that the 'anomaly', in the literal sense, still yields a safe and satisfactory structure, and yet is felt to deviate from what could be achieved, for instance by being too safe, or more costly than necessary, or by taking longer to build than necessary. It is in the nature of commercial construction engineering that such events are not likely to become apparent since each design is unique and there is seldom time or reason to investigate investigate every case; also, when this sort of anomaly does arise, a designer is most likely to use the discovery to produce a different design which is less over-safe or cheaper or quicker to build. To borrow again one of Kuhn's phrases, this sort of activity, which is very much part of periods of 'normal' design, is what he calls 'paradigm articulation'—anomalies are explained and design procedures modified with little or no need for a fundamental rethink. This is ultimately part of the practice of 'normal' design. The problem of anomalies becomes serious

only when they cannot be explained—especially when a resulting failure has been particularly costly or the cause of injury to people.

14.4 CRISIS AND REVOLUTION

A state of crisis in the revolutionary cycle arises when there has occurred one or more anomalies which cannot be explained using the current understanding of the design procedures and the scientific theories and other knowledge upon which they are based. As noted in the previous section, the most obvious way in which crisis might arise is as the result of a failure of the design procedure to produce the expected outcome, and there being no accounting for why, or rather there might be some understanding of why, but no known way in which such an understanding can be incorporated in a new design procedure. The collapse of the Brighton pier in 1836 was clearly due to excessive wind, but there was no way of modifying the then current design procedures for suspension bridges to take account of such conditions and ensure a safer bridge next time. Some examples of crisis resulting from failure will be discussed in the next section.†

Alternatively, engineering designers might face a state of crisis without there having been a failure. This may be because there had arisen a body of experience which led to doubts about the effectiveness of a design procedure, for instance in not being sufficiently rational to provide a plausible justification of a proposed design, or producing designs which were unnecessarily over-strong. Crisis may also arise simply because, at a particular time, there was no known design procedure which could adequately justify the proposed design of a new type of structure, such as was the case when the 140-metre spans of the Britannia Bridge were proposed in 1845. Several examples of this type of crisis—crisis resulting from contemplation, will be discussed after looking at crisis resulting from failure. But first a brief word about the role of failures in history.

Failure, especially collapse, has acquired a special, though somewhat over-rated, importance in the history of engineering. It has been the subject of a great many books and studies and it is often implied that history should be studied principally because of the lessons which can be learnt from past failures. Structural failure is generally acknowledged to be a stimulus for both change and progress in the development of structural engineering. Indeed, it is widely supposed to be *the* stimulus for change [Smith D.W. 1976; Sibley & Walker 1977]. The phrase *trial and error* embodies this belief, although this now usually associated with engineering development in the days of 'pre-scientific' design. This mechanism for progress is still acknowledged to be important nowadays, dressed up, perhaps, in more philosophical (and respectable) clothes, such as 'careful experimentation' or 'conjecture and refutation' [Blockley & Henderson 1980]. However, there is nothing necessary about failure having to lead to crisis, change and progress. If there were, then there could be no progress without failure. The problem with assuming a logical relationship of the type:

'a failure leads to a change of design procedure and, hence, progress'

† The words 'anomaly' and 'crisis' do sound rather melodramatic when applied to situations which many engineers consider to be relatively everyday; I have chosen to stick with them to preserve the coherence of Kuhn's philosophy and historiography.

is that it excludes relationships such as:

'a failure does not lead to progress', and

'something else might lead to progress'.

In identifying the anomaly and crisis (rather than failure) as precursors of change, we avoid the trap of believing that failures are a necessary precursor to change.

14.4.1 Crisis resulting from failure

At a particular time in history, a current design procedure will incorporate, often implicitly, a number of failure criteria which are believed to be pertinent. This belief is one of the paradigm beliefs of the particular design community. So too is the judgement that a newly proposed structure is 'similar in relevant respects' to a previous one, on which the chosen design procedure had been used with success. However, an apparently insignificant change of circumstances may lead to a failure. This may be a wholely new kind of failure or a kind already known in other types of structure or in a different field of engineering or at an earlier time in history; all indicate that the new structure was not, in fact, 'similar in relevant respects' to previous ones.

This type of failure typically occurs when an existing design procedure is extended to larger or new types of structure and previously insignificant effects become significant. This is perhaps the most common reason for failures occurring. Such an event becomes increasingly likely when details of a design procedure, such as the assumptions upon which it rests and the limits of its application, have been poorly recorded and disseminated through a design community, or even forgotten. A failure of this type, however, even a dramatic or catastrophic one, may or may not lead from a mere case of anomaly to one of crisis. *Different failures are not equally significant in the historical development of engineering* and their significance does not depend on the death toll.

One possible outcome of the 'post mortem' on a collapsed structure is that a series of relatively minor points will come to light all of which are more or less understood, and existing design procedures can be easily modified (assuming it was a design failure)—a case of paradigm articulation. The following are examples of this set of circumstances.

Tay Bridge collapse

Although this famous structural accident in 1879 has been discussed many times by historians of engineering, its historical significance seems to derive mainly from the horrific death which befell the 75 people in a train when the bridge collapsed. The accident had very little effect on subsequent bridge design. Apart from evidence of poor detailing and quality of construction, the main design defect was the adoption of a low value for the force exerted by wind on the bridge girders and piers. The chosen figure for an average wind load of 20 lb/ft^2 was widely held to be an acceptable value by contemporary engineers, and instantaneous peaks of 40 lb/ft^2 or more were considered to be irrelevant to the overall stability of a large structure, despite a lack of reliable experimental evidence to justify these feelings. There were, however, others who believed the peak values were the ones which designers should use

(Smeaton: 50 lb/ft^2; Rankine and many continental engineers: 55 lb/ft^2) [Sibly and Walker 1977]. The only change which followed this accident was a general raising (to 56 lb/ft^2) of the wind load figure which designers should use—a prudent, but hardly radical change.

Ronan Point

In 1968 a 22-storey block of flats at Ronan Point in London suffered a severe progressive collapse following a relatively small gas explosion in a kitchen on the eighteenth floor. This mode of failure was easy enough to imagine, and commonly considered in design procedures for other types of structure but it had not been considered as a possibility in the particular case of buildings like Ronan Point. The accident occurred not because this type of failure had not been considered—it was not considered in other blocks of flats either; it happened because there had been a change to the structural system. A similar explosion in a steel- or concrete-framed building would probably have blown out the non-load-bearing cladding of one or two rooms. Ronan Point, however, was constructed with load-bearing, precast concrete cladding panels, removing one of which would lead to disastrous progressive collapse. Again, once the problem had been exposed, relatively minor changes were required to design procedures to render them safe again.

Tyne estuary electricity pylon

In the 1960s a 600-ft pylon carrying electricity cables over the River Tyne collapsed. It had been designed by extrapolating design procedures developed for much smaller structures. Although the individual members were strengthened and stiffened in proportion to their greater size, the joints were not modified in the same way. They thus became relatively less stiff and allowed a buckling mode of failure which had not been previously considered. In the smaller exemplars, the joint stiffness had been known to be ample and had not featured as a major criterion in the design procedure. The cause of the failure was relatively simple to establish and the design procedure for similar structures easy to make safe again.

It may, however, transpire that a serious structural failure cannot be explained and, despite detailed investigation, no solution to the problem is found. In this case the only option is to avoid the crisis by ceasing to build similar structures. This was effectively what happened with suspension bridges after the 1830s in Britain.

A similar fate befell the cast iron girder bridge after the collapse of the Dee Bridge in 1847. In the early days of the railways, cast iron girders were usual for small spans and to increase the spans it had become standard practice to add wrought-iron trussing rods to overcome the inherent weakness of cast iron in tension. We do not know how or why Stephenson's bridge failed, but a likely explanation is that, as spans increased so the slenderness increased and a new mode of failure became a possibility—the lateral–torsional buckling of the girder, perpendicular to the spanning direction: the chances of such a failure would have been aggravated by the lack of trueness of the three 30-ft castings which formed the 90-ft span and the asymmetric way in which the girders carried their load. The existing, well-tried, design procedures for such bridges had not included this mode of failure as one which needed to be considered in design, although the phenomenon of lateral–torsional buckling was known

[Hodgkinson 1831:463]. Previous examples of this type of bridge had just happened to have sufficient lateral stiffness. Thoughts of lateral instability were not part of the current paradigms of either engineering design or science. The subject was not even mentioned during the subsequent inquiry. At that time, thoughts were more highly attuned to what we now call brittle fracture and fatigue problems which were affecting many components of steam engines and the bridges and rails upon which many of them ran [Timoshenko 1953:162ff]. It is not surprising, then, that the Royal Commission set up to investigate the accident concentrated mainly upon the effect on cast iron girders of 'concussions, vibrations, torsions and momentary pressures of enormous magnitude, produced by the rapid and repeated passage of heavy trains' [Report 1849]. The Commission came to no clear conclusion as to the cause of the accident and the resulting uncertainty undoubtedly led to an accelerated switch to wrought iron as a structural material.

Finally, and, in the present context, most interestingly, there is the third possibility that a structural failure might initially not be understood but, after a programme of investigation, a radically new approach to design is established. Sometimes the result is that a particular engineering design community finds it has to incorporate new ideas into its design procedures which were previously not believed to be significant, although the ideas themselves had not previously been totally unknown. Such design revolutions are not accompanied by scientific revolutions—rather, existing scientific knowledge is adapted or developed to be of direct use to the design engineer by means of experimentation to understand phenomena better and to use this understanding to propose and test a new approach to design. This was just the case with the Tacoma Narrows bridge collapse discussed in §13.4—it was striking that the first explanation of the disaster was provided within two weeks from the pen of an expert in aerodynamics, a subject in which bridge engineers had previously had had no great interest.

A similar story can be found in the case of a number of steel box-girder bridges which collapsed around 1970. In one sense, the cause of the accidents was that the large spans of the bridges and their construction, by building out from one support pier to the next, led to larger-than-anticipated compression stresses in the thin steel plates, which subsequently buckled. The slenderness of the welded plates had become increased to such an extent that normal-sized initial deformations in the plate became significantly large and provided the mechanism for local buckling to occur. However, the problem caused by initial deformations was not unknown—it was already well-known in the aircraft industry and had even been mentioned by Thomas Young around 1800. Rather, the problem was new to this type of welded steel structure and had previously not been included in the design procedures. The problem was particularly highlighted because of longer spans and a great many factors concerning the management of the construction of the bridges, including the choice of method [Blockley 1980]. The subsequent investigation exposed considerable shortcomings in the way bridge engineers usually checked the stability of the slender steel webs of box-girders. Assumptions about the amount by which the plates might deviate from being truly flat were found to be over-optimistic and, in looking at the distribution of shear stresses in the web and flanges of the beams, designers were making approximations appropriate to relatively shallow beams, not deep ones in which the phenomenon of shear lag becomes significant. The result was a revolu-

tion in design procedures for steel bridges which resulted in a new British Code Of Practice (BS 5400).

Occasionally in the history of structural engineering there have been revolutions in engineering design which have come about as a result of several unexplained accidents, followed by a scientific investigation leading to the discovery of an entirely new phenomenon in engineering science and a corresonding scientific revolution. The two most dramatic of these have concerned metal fatigue and the brittle fracture of ductile materials.

Although it had already been recognized by practical engineers for many years, it was only in the early 1840s that any serious scientific investigations were made into the phenomenon by which a metal could fracture by the repeated application of loads well below its usual tensile strength. Following a series of unexpected and catastrophic fractures of the axles of railway locomotives, Rankine presented one of the first papers identifying the phenomenon, in 1843. Although he did not achieve an explanation, he was able to recommend a better design in which sharp included angles were avoided in favour of fillets with larger radii (to reduce stress concentrations, as we would now say). During the decade which followed, which was at the height of 'railway mania', engineering scientists all over Europe were experimenting in all manner of ways to establish the vulnerability of materials to repeated loads [Timoshenko 1953:162ff]. In addition to cyclic loading, many tests also involved varying the degree of suddenness with which the loads were appied (due for instance to different train speeds), since it had also been found that the rails and bridges over which the locomotives passed could suffer the same problems as axles. It was in the midst of this crisis about fracture that the Dee Bridge collapsed—no wonder everyone was looking for an explanation of this collapse which involved fatigue or a related brittle fracture.

By the 1860s, enough experimental information had been gathered to enable the designers to incorporate fatigue into new design procedures. In the meantime, however, it is worth noting other ways in which the engineering designers had dealt with their crisis. As well as avoiding sharp-cornered notches, safety factors used in the design procedures when calculating cross-sections of metals were increased, quality control in production and assembly was improved, components were subjected to more frequent inspection for the onset of fatigue cracks, and, wherever possible, the more vulnerable cast iron was replaced by wrought iron. In the case of the axles of locomotive drive wheels, a particularly ingenious solution was found. A major cause of the vibrations had been the asymmetry of the drive wheel due to the large joint with the connecting rod used to turn the wheel. These vibrations were, to a large extent, eliminated by incorporating balance weights into the wheel rim to reduce the dynamic asymmetry—the elegant solution of a practical engineer.

More recently, there has been a second revolution in engineering design and science in the field of brittle fracture. The problem initially manifested itself as a series of dramatic failures of welded steel ships and bridges in the 1940s. These had occurred in circumstances which could not have had normal metal fatigue as the cause. Then, in the early 1950s, two Comet aircraft (the first passenger jet) appeared to explode in mid-air. After an intense programme of research, these (and many other) brittle fractures of what are usually ductile materials, were found to be examples of the same phenomenon. The conventional approach to fatigue focused

only upon the macroscopic behaviour of brittle materials in a fundamentally empirical way and could offer no explanation to this series of disasters. The crisis for engineering designers became one for the engineering scientists as well, and two revolutions followed.

In the investigation of the Comet disasters it was found that a ductile material can behave in a brittle manner under certain circumstances which could only be understood by looking at cracks in a radically new way—at their microscopic behaviour and the way they propagate through materials. It was found that crack propagation was governed by the amount of elastic energy stored in the stressed material—the greater the stored energy, the smaller the crack which will propagate in a brittle and sudden way. The change from rivetted steel ships to all-welded ones had the twin effect of introducing cracks of the critical length into the ships' structure and simultaneously creating steel which was brittle at low temberatures by virtue of the unintentional heat-treatment caused by the welding process. A similar combination of events exposed the same problem in the Comets—the change to larger and higher-flying passenger aircraft simultaneously introduced pressurized interiors, a lower pressure outside the aircraft and 'cracks' of virtually the critical length throughout the more highly stressed fuselage in the form of sharp-cornered, rectangular windows. The solution was found in adopting the smaller, round-cornered windows which are familiar today.

14.4.2 Crisis resulting from contemplation

Although less dramatic than crisis resulting from one or more spectacular failures, some of the most significant revolutions in engineering design have come about by careful thought and testing by means of experiment. For a variety of reasons some members of a design community have come to feel that a design procedure can be improved. This feeling comes about as a result of a sense of dissatisfaction, especially with the power of a design procedure adequately to justify a proposed design or with the ability of a design procedure to yield a design which is as economical or safe as the designer believes to be possible. Such feelings have their origin in the designer's intuitive understanding of structural behaviour, action and adequacy (see §7.1).

Precisely what a designer believes is extremely subjective. It depends both on the individual and his particular experience, and on the particular design community to which he belongs. It depends on the widespread beliefs of the day—the current paradigms—and what is considered 'normal'. It depends on the 'engineering climatology' and on the aspirations of the age—while rationality and economy are perhaps the most dominant characteristics of our own age, they were not necessarily the most important in other ages and, indeed, what is actually judged to be rational or economic also varies from age to age.

So it is that during the entire history of civil and structural engineering design that there has been a continuing drive to improve the means by which a design procedure can discharge its twin functions of description and justification. We have already met the example of the plastic design revolution which arose out of nothing more than a feeling that current design procedures were not rational enough and probably not leading to the most economic structures. In another context we met another important revolution which did not have its origins in structural accidents—Rankine's 'harmony of theory and practice' (see §4.2).

Rankine's revolution might also be called the 'safety factor revolution', for his brilliant achievement was to change the very meaning of this notion and put it to an entirely new use. Before the mid-19th century the safety factor had meant just that—a factor by which a given structure or element was stronger than the load which would cause collapse. Rankine was passionately concerned to get practical design engineers to make use of the valuable and, in particular, *rational* work of the engineering scientists. The designers and builders were extremely reluctant to incorporate much of this great body of knowledge for what they believed to be extremely good reasons—the scientists worked in controlled, ideal conditions which did not reflect the reality of actual structures being built. In an age when empirical testing of structures was so common, practical engineers possessed a wealth of first-hand experience which the scientists did not.

Rankine carefully showed how actual structures, beginning with the simplest elements such as beams and columns and working up to assemblies of elements, could be represented by idealized versions (mathematical models) of these elements and discrepancies between the behaviour the model and the real world could be taken into account by means of an empirical number which would still be decided by the practical engineers. A design procedure would consist in calculating, by rational means, the dimensions of an ideal structure and multiplying the results by the empirical number which resulted in the practical engineer choosing virtually the same beam (or column etc.) as he would have wanted to use anyway.† Thus was Rankine able to inject sufficient rationality into design procedures to satisfy his own concern about purely empirical methods, while managing to incorporate the valuable empirical knowledge of experienced practical engineers. Perhaps also to ease the adoption of his ideas, he continued to use a familiar phrase, the 'factor of safety', although the sense in which it incorporated safety was utterly new. More recently, incidentally, the concept of the 'factor of safety' has undergone further, more subtle change which gives some cause for concern. Although it is still seen as a measure of the discrepancy between the behaviour of the mathematical model and the actual structure, it is nowadays seldom acknowledged openly to be an empirical number which has been established by tests and experience. Rather, it tends to be considered as the amount by which the behaviour of an actual structure deviates from the behaviour of another actual structure which behaves in precisely the way the mathematical model predicts. In other words, there now exists in many people's mind the idea of an actual structure whose behaviour can be predicted perfectly and that safety factors ought, more and more, to tend towards unity.

A state of crisis in a design community can arise in one final way—it might be required to design a type of structure which has not been designed or built before. In such cases a designer can be rather at a loss to know how to start since, with no precedent, there is no way of knowing what the limiting conditions of service might be, or what the imposed loads might be, or which failure mechanisms are likely. The first stage of such a problem has to be to collect some data to break the vicious circle, by seeking some sort of precedent, even if only remotely similar, either in reality or by making models. This very problem arose in the case of the first nuclear power sta-

† Precisely the same subterfuge was used by Baker and others to help get their plastic design methods accepted (see §11.5 and §14.5).

tions. The reactor cores had to be safely encased to resist explosion. Since there was little idea of the sizes of forces involved and little chance of carrying out full-scale tests to establish actual failure mechanisms, the designers had to draw largely on experience of other types of explosion and model tests.

A similar problem was encountered when a manufacturer of cars approached a firm of structural engineers for assistance with the design of the structural frame of a new car. By and large, the load-bearing structure of cars has previously been derived entirely empirically: they were designed in the light of the company's previous experience and by testing them both on the road and in a large variety of collision tests. At no stage did this empirical approach involve establishing the precise loads acting on a car during cornering and in collisions, nor the upper and lower limits of stiffness and strength required for adequate performance and safety. From the structural engineer's point of view, and according to the design procedures usual within *their* design community (their accepted paradigms), the problem was effectively an entirely new one; the paradigms of the car design community were of little help.

Back in the realm of architecture, the 1960s saw the virtual birth of a new type of structure in the tensile net and fabric structures associated with Frei Otto. Although Otto was an architect, he helped to bring about an engineering design revolution which made large tensile structures possible for the first time.

Tension structures in the form of nomads' tents are of ancient origin and modern circus tents and marquees are greatly enlarged versions of these. The geometry of the fabric surfaces is relatively simple and, since stresses in the fabric are relatively low, the fabric is not stretched significantly when the tent is erected. Design is therefore a simple matter and wholly empirical. The first large tension structures built in the early 1950s were based on grids of steel cables on which were supported cladding panels of steel or concrete, or which effectively became the reinforcement in a thin reinforced concrete slab. Whatever the architect's wishes, the design engineers were severely restricted in the shapes which could be designed. Some roofs were simply draped in a catenary between two rows of supports. Others, such as the Raleigh Arena, were doubly-curved, anticlastic surfaces, which allowed the two orthogonal sets of cables to be prestressed to give the roof its required stiffness. However, the geometry of such surfaces was still kept relatively simple (a hyperbolic-paraboloid) since the engineers could only justify the stability of surfaces whose geometry they could describe mathematically.

Otto was strongly impressed by the possibilities of doubly-curved, prestressed surfaces. He started experimenting with a variety of models to generate a wide range of curved surfaces formed by chains and nets in tension. Some of these were direct descendants of the funicular shapes (Hooke's hanging chain) used in the 1740s by Poleni to establish the best shape for the dome of St Peter's and in the 1890s by Gaudí for the design of his cathedral in Barcelona. But Otto also experimented with a new type of structure which relied on the tensile forces within a sheet of fabric, taking his inspiration from the shapes of soap bubbles. Using elastic sheets and a variety of nets including ladies' stocking material, he was able to generate an enormous range of natural shapes which had no convenient mathematical equations—they were, literally, inconceivable in mathematical terms and hence could not be analysed at all by engineers. It was particularly frustrating to an architect to be able to conceive and generate beautiful shapes, in miniature, which the engineers were not able to design

at the sizes required for their use as buildings. This state of crisis was resolved by taking a radically new approach and devising design procedures very different from those used to design the engineers' large cable tension structures, such as the Raleigh Arena. A new type of design procedure had to be developed which relied entirely on models.

There were two problems which had to be solved. The first was to develop the technique of making models which were believed to be a suitable representation of the sort of materials and structure and which would be used in a full-size building. This was achieved by experimenting with three types of material: a variety of inelastic chains, nets and fabrics giving models whose geometry was governed only by statical considerations, elastic sheets, and the purely plastic material of a soap film (the surface stress in a bubble is independent of its size) [Bach *et al.* 1988]. No one of these model materials was found to be an adequate representation, and often all three types of model had to be made to generate different types of information. The soap film models presented the greatest technical challenge—no-one had ever made bubbles up to a metre across and which would last long enough to be studied and measured (soap solution was continuously fed into the bubble to balance evaporation losses). The second problem was to devise ways of accurately scaling up the geometry of the model, perhaps several hundred times, in order to provide the material and connexion specialists and the fabricators with sufficient information for them to be able to achieve the desired results. A variety of techniques was developed to enable the complex geometry to be accurately surveyed. Chains and nets were not too difficult, but fabrics, elastic sheets and bubbles required the drawing or projecting of regular grids onto the surfaces as well as the use of careful photography (Fig. 14.1). Sometimes a solid cast of the shape made by a fabric or elastic sheet was taken to allow small pieces of flat material to be laid on them to help plan the best cutting patterns for the tensile material in the full-size structure.

The justification of the proposed designs was, in one sense, achieved simply by demonstrating that the model structure would work—the very geometry of the tensile shape is governed by the statics and elasticity of the materials of which the model is made. There remained only the not inconsiderable problem of scaling, by a factor of perhaps several hundred, the anticipated loads and elastic properties of the materials between the model structure and the full-size version, and vice versa. This was achieved and a number of trial structures (spanning only a few metres) were built and tested in order to prove the reliability of the design procedure. From that point, designs progressed rapidly to the 40-metre spans of Otto's German Pavilion at Expo '67 in Montreal and the 135-metre spans of the roof over the sports stadia at the 1972 Munich Olympics. These structures had only become possible by developing an entirely new type of design procedure which avoided the impasse inherent in the previous, wholly analytical design procedures.

These few examples have illustrated how progress in engineering design can be achieved without the 'need' for accidents implied in the concept 'trial and error': progress is also possible by means of 'trial and success'. This latter notion corresponds better with the fact that engineering designers *always* intend their designs to be 'successes' (and always have done). If progress in engineering were to be by means of trial and error, then it could only ever be accidental, since, unlike engineering scientists who conduct experiments to try to falsify their hypotheses, engineering desig-

ners never have any intention that their structures might falsify a design procedure. Designing a building or any other artefact is, in no way, an experiment. For a human action to be an experiment, it must be carried out intentionally and with the possibility of several outcomes, including failure.

14.5 CRITERIA FOR APPRAISING DESIGN PROCEDURES

We have now looked at the circumstances which can lead up to an engineering design revolution. There follows the post-revolutionary phase, when a new type of design procedure is in competition with an older one, and a new design community is establishing itself. There remains the question as to how the new design procedures gain acceptance, especially among adherents of the older procedures. It is tempting to assume that it would simply be a matter of logical argument. However, this can only occur if both sides agree on basic concepts, the validity of assumptions made and fundamental aims. But we have seen from some of the examples of design revolutions that the whole way of thinking after a revolution is different from, and incompatible with, the way of thinking prior to the revolution.

Although each design community will be able to present a logical argument as the justification of a design, different communities will differ in the concepts and the language they use, the historical evidence and precedent which they believe to be significant, the particular branches of engineering science which are important, the type of mathematical analysis which should be used, and so on. Given these differences in the whole way of looking at their problems, while each design procedure may be self-consistent, it will not be possible for members of different communities to settle their differences wholly logically. Each community would argue that theirs is the 'best' or 'correct' or 'proper' way to design and that others were illogical or unsafe or costly or, in some other way, failing to meet the agreed and self-imposed design criteria of that particular community. Put another way, there is always a degree of subjectivity in the choice and application of design procedures and it is this individuality which characterizes different design communities. When it comes to two individuals discussing the relative merits of alternative design procedures, each may be entirely consistent and neither could be accused of being wrong, yet there would be no way of using reason to resolve their differences. As Kuhn said about the corresponding stage of a scientific revolution:

> there is no neutral algorithm for theory choice, no systematic decision procedure which, properly applied, must lead each individual . . . to the same decision. [Kuhn 1970a:200]

Ultimately, each design community will have its own set of criteria for judging the quality of design procedures and answering the question, 'What are the characteristics of a good design procedure?'. Curiously, despite the clear importance of such a question, very little indeed has been written in answer to it. A related problem has also received little attention—the problem of deciding which of several possible design procedures (including the mathematical models and techniques of structural analysis) would be most suitable in a particular case.† The answer is often given, or

† For rare examples of discussions of both these issues, see [Cross 1935a, 1935b; Baker 1936/37; Brown 1970; Chilver 1976; Smyth 1969].

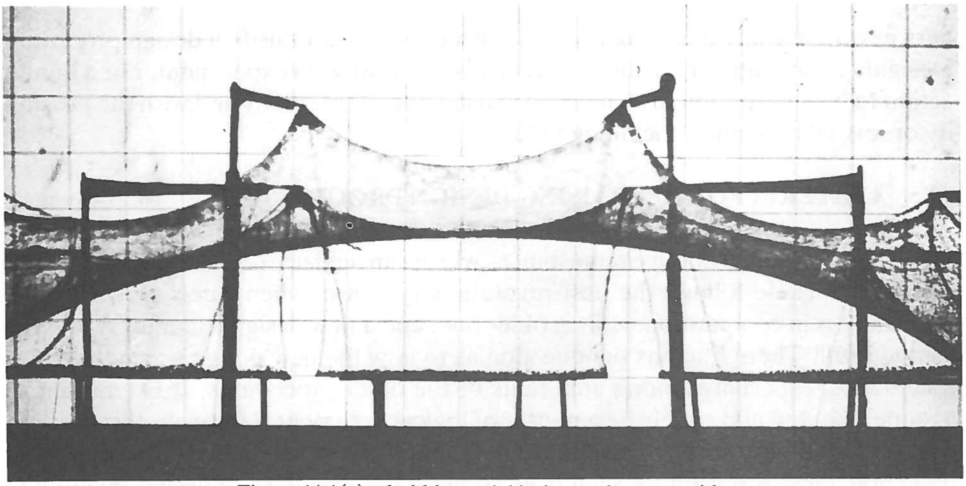


Figure 14.1(a)—bubble model in front of square grid

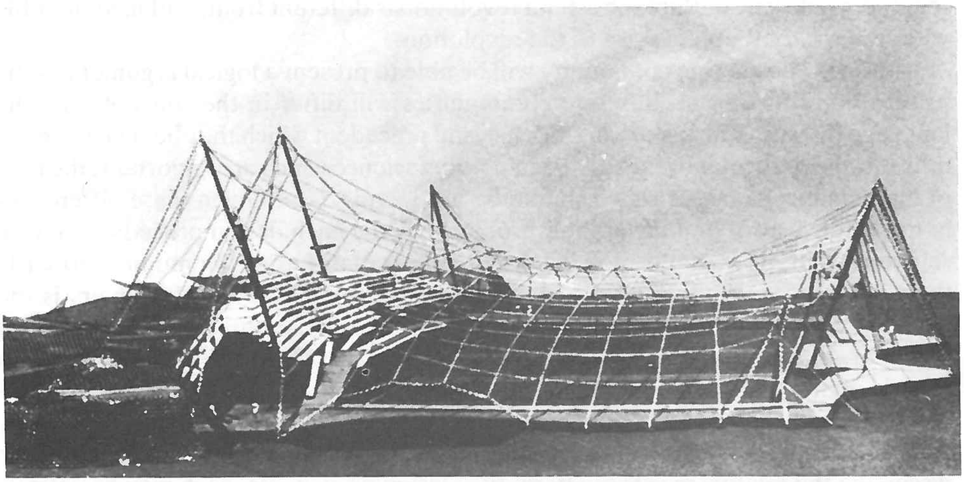


Figure 14.1(b)—chain model (inverted)

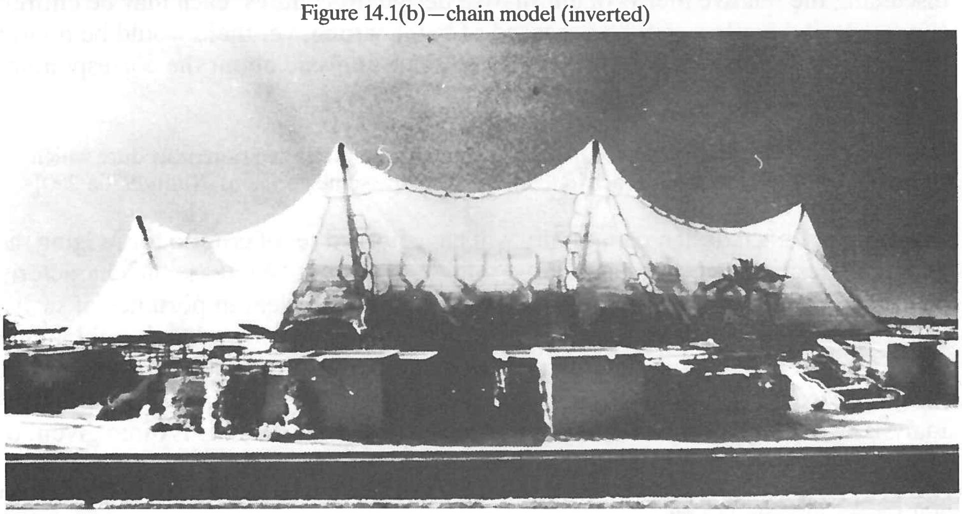


Figure 14.1(c)—fabric model

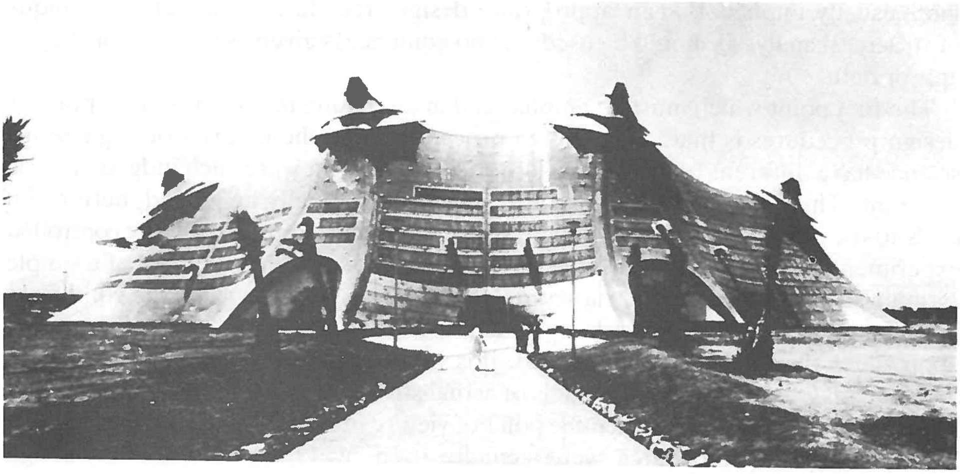


Figure 14.1(d)—Jeddah sports hall

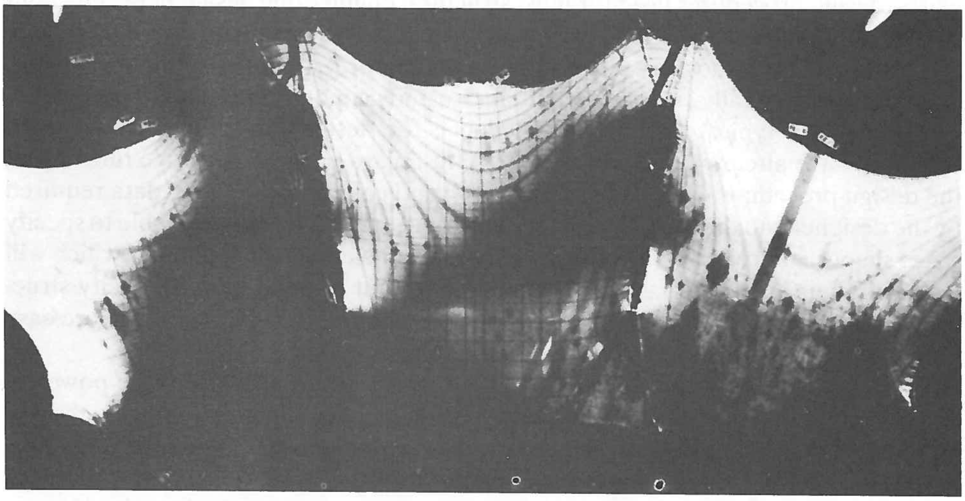


Figure 14.1(e)—shadow grid projected on fabric model

Fig. 14.1—Various models used in the design of tensile structures (a) Form-finding bubble model for Jeddah sports hall. (b) Form-finding suspended chain model for Jeddah sports hall (inverted), based on bubble model studies. (c) Final polyester fabric model for Jeddah sports hall. (d) View of a finished sports hall in Jeddah (Engineers: Buro Happold and Ove Arup and Partners, 1981). (e) Fabric model of the Munich Olympic stadium. The shape of the surface is made visible by means of a projected shadow grid. (Engineers: Leonard & Andra, 1972; model studies: Institute for Lightweight Structures)

more usually, implied, that an 'appropriate' design procedure (or model or technique of structural analysis) should be used; yet no guidance is given as to what constitutes appropriate.

The first point which must be emphasized in discussing the criteria for appraising design procedures is that, since design procedures and the theories of engineering science have different functions, the criteria by which they are each judged are also different. Thus, for example, a scientific theory will usually be judged 'better' if it leads to the more accurate prediction of stresses and deflexions in carefully controlled experiments, whereas this is not the case for design procedures. The use of a simple formula such as *WL/8* may be successfully used in design, even when the conditions in an actual structure are not appropriate to those pertaining to the scientific theory upon which the formula *WL/8* is based. It is very common for design procedures to be based on grossly simplified models of actual structural behaviour. Indeed, utterly 'incorrect' models (from the scientific point of view) can be used in design with success. Whatever design procedures were actually used by Greek and Gothic design engineers, or even by Christopher Wren, they would be judged nowadays as 'incorrect' on engineering scientific grounds, yet they were used with success. Similarly with the elastic design procedures used for most structural engineering design between about 1830 and the present day—the conditions in an actual structure are usually considerably different from those assumed in the mathematical model. Yet, the design procedures were generally successful, in that safe and economic structures were built.

So what are typical criteria of excellence for design procedures? Safety and economy have already been mentioned. With regard to the descriptive function of the design procedure, convenience of storing and communicating the data required by the designer and the constructor are important. It is important to be able to specify sizes, shapes and relative dispositions of structural elements in a manner which will facilitate their justification. It will also be important to describe and specify structures in a way which makes them easy to manufacture and erect, and which are easy to verify as being constructed as the designer had intended.

Turning to the justification function of the design procedure, the more powerful and convincing the justification, the better. This may be achieved simply by the use of a very well-tried procedure. However, if an argument making use of deductive logic is used, then the more plausible and rational the procedure the better, particularly concerning the assumptions upon which it is based (since the logic itself is not open to debate). It will be recalled that the initial aim of the Steel Structures Research Committee was primarily to develop *more rational* elastic design procedures. This was Rankine's aim too.

The power of the justification discharged by a design procedure also relates directly to the safety which its use is supposed to ensure. In the light of this, it is interesting to note the recent introduction of probability and statistics and so-called partial safety factors into the assessment of structural safety. Ultimately, a probability is a measure of the power of justification (the level of confidence or the degree of belief) in a design procedure. In this respect they resemble the betting odds which give a measure of a gambler's level of confidence or degree of belief.

Finally, in this brief consideration of the characteristics of 'good design procedures', there is the suitability of a particular design procedure to the working practices of engineering designers themselves. A design procedure must be acceptable to

the profession, in terms of both its complexity and the effort expended in using it. It must also reflect the fundamental beliefs of the particular design community. For instance, both ease of use and speed are important to the designer. It was these two requirements which led to the the engineering design profession rejecting the new, more rational, elastic design procedures developed by the SSRC in the late 1930s. The technical press is nowadays as full of complaints by practising engineers about the increasing complexity of new design procedures and Codes of Practice as it was during the last century. As well as the effort of using a design procedure, in itself, there is also the effort as seen in the light of the reward it may bring to the final design. This criterion, too, contributed to the design community's rejection of the new procedures developed by the SSRC: they created neither cheaper nor safer buildings.

As a rather less specific criterion of appraisal, there is also the inherent conservatism of design communities. The reader may recall the example (in §11.5) of the introduction of the load factor to elastic design procedures 'by subterfuge' in order to overcome the reluctance of some members of the SSRC to change. Similar 'unscientific' methods lay behind the Committee's choice of a load factor of 2.36 in the new rational design procedures for struts:

> the excuse for this extraordinary value was that it made the strut curve nestle safely in the middle of those already in use elsewhere. [Baker 1954:15]

A similar means of encouraging acceptance lay behind the choice of a load factor of 1.75 for use in the plastic design procedures proposed by Baker to replace the elastic procedures. This value was chosen because its use would lead the designer to select the very same I-beam, under similar loading conditions, whether the old elastic or the new plastic design procedures were used [Baker *et al.* 1956:44 and 352ff].

In conclusion, it should be emphasized that there is a significant subjective element in both the choice of, and the relative importance assigned to, the various criteria for appraising design procedures. The same was seen to be the case for scientific theories (§9.4). In the field of structural behaviour, the subjectivity is often masked by the use of calculations based upon scientific theories which are believed to be both 'true' and extremely accurate:

> The word 'calculate' merely gives an undeserved impression of authority to a disguised guess. And the better the disguise the greater the danger. Sometimes we tend to discount the guesswork element in our own picture thinking of how a mechanism actually works, while being very critical of other people's subjective ideas. [Glegg 1971:79]

The tendency to believe that decisions about designs and design procedures are reached as a result of clear logic (and are therefore immune from criticism) is accentuated by the very need for designers, after a design is complete, to provide a logical justification for it. It is often forgotten that the design was probably not reached by that route. This is an example of what has been called the 'rational fallacy'—the presentation of information and arguments with the benefit of hindsight, and tends to suggest that a particular solution is 'the one best way' (see Fores & Sorge 1978). It also makes it frequently (and mistakenly) appear that engineers' designs have been based on science—the name 'Project Hindsight', given to an investigation by the

USA government into the application of science in technology, reflected this observation [Layton 1971].†

14.6 MANIFESTATIONS OF DESIGN REVOLUTIONS

Having earlier discussed the invisibility of design revolutions, it would be a fitting conclusion to highlight the particular ways in which they do manifest themselves. Sometimes the effects of a design revolution are clear for all to see: there are physical changes to the way structures are built; while other design revolutions have a more subtle impact which often cannot be seen from the structures themselves.

There are several reasons why the design of a structural type might change. It may be due to the introduction of new materials such as cast and wrought iron or reinforced concrete, or to the development of improved material properties such as strength and corrosion resistance, or to improved manufacturing methods which can lead to higher quality materials and the availability of larger sizes or new shapes. There may be new techniques of assembly and joining components, such as the development of rivets or of welding. Very occasionally a new type of structure is invented: the hyperbolic-paraboloid shell; or a new structural action: prestressing.

In addition to all these types of change, some of which are signs of other revolutions, in engineering science or technology, there are also changes which come about due only to new approaches to design. In the previous chapter we saw how the design of trusses changed during the 19th century as a result of several design revolutions. What changed was the way in which designers *saw* trusses, and hence how they should go about designing and building them—first, as beams, and later as statically determinate, or indeterminate frameworks. We also saw how the arch, too, underwent a series of changes according to how people *saw* it—how, in some fundamental way, they understood it and sought to incorporate this understanding in the way they designed arches. With the introduction of the pin-joint, both in trusses in the early 1850s and arches in the mid-1860s, we see major evidence of structures being built such that they are made to behave in the way a designer has conceived. This shows a markedly different approach from earlier design procedures when the design of structures was influenced largely by empirical and practical matters. It reflects a radically new way of designing in which the need to justify a proposed design dominates all other considerations.

Rather more frequently, design revolutions have occurred without obvious influences on the final appearance of structures and this helps to explain why revolutions are often so difficult to identify. This has been especially true for those revolutions which have concerned the different mathematical models which a designer has chosen to represent a structure, the materials of which their structures were made and the loads which acted upon them. It is, for instance, impossible to see from the appearance of a steel portal frame whether it has been designed using plastic design procedures or elastic methods. Similarly in the case of arches in the 18th century—we

† It concluded, by the way, that technology was not 'applied science' and technical progress was not fuelled by scientific research.

cannot discern simply from outward appearances, the transition from empirical design to design procedures involving a rigorous consideration of statics.

In the case of materials we cannot see that, during the 17th and 18th centuries, for instance, materials were thought of as either inelastic and brittle or elastic and ductile (tough). Brittle materials such as stone were believed not to deform elastically before fracture. In the 1860s the idea of fatigue and brittleness were incorporated into design procedures. In more recent times, there has been a return to seeing materials as non-elastic in some circumstances—the stability and ultimate strength of certain structures can be investigated by considering collapse mechanisms with 'plastic' hinges having a certain resistance to rotation (nil in the case of masonry structures). Nor can we recognize in bridge design the changes in the way loads were taken into account in design procedures. Before the early 1800s the loads acting on bridges had usually been considered to be uniformly distributed and the bridge's self-weight was not considered. With the dawn of the railway age, large concentrated point loads were first considered and following the fatigue crisis of the 1840s design procedures began to include impact loads caused by a train's speed. Later the effect of a series of moving point loads corresponding to each pair of train wheels was summarized in an 'influence line' for each structural member so that a better estimate of a bridge's fatigue life could be made.

This sort of information can really only be found in a designer's calculations and attitudes common to whole design communities as expressed in contemporary scientific papers and textbooks for engineers.

It is in the written word that we find the best evidence of design revolutions which do not manifest themselves in finished structures. As was discussed in Chapter 7, the way engineers think about structures depends upon the concepts with which they are familiar and in terms of which they *see* the structures and interpret how they are working. We have also seen that such concepts constitute the paradigms which help to identify and characterize a particular scientific or design community. A revolution is marked by a change of these paradigms and the establishment of a new community with new beliefs, concepts, language and vocabulary.

It follows from this that the occurrence of an engineering design revolution will be marked by changes in the language of engineering—the coining of new words or using old words in new ways. We have already met this in connection with the plastic design revolution and concepts such as 'plastic hinge', 'proportional loading', 'collapse' and 'shakedown' (§11.5). Some other brief examples will show how engineers have described their world differently in different ages, reflecting how they saw the structures, the problems faced in designing them and, in recent history, the mathematical models by which they could be represented:

- The development of the notion of a 'truss' from a means of strengthening and stiffening a single beam, to an assembly of many short members to form a whole roof or bridge structure, to the modern sense, implying a structure in the members of which the principal forces are axial.
- The change from using the word 'arch' to describe the (geometric) shape of a structure, to its use implying a certain type of structural action involving, primarily, compressive forces.
- The change from using the phrase 'suspension bridge' to describe any bridge,

including truss bridges, in which some members carried significant tensile loads (e.g. [Cresy 1865]) to its modern use.

- The changing meaning of the word 'failure', from collapse to exceeding a limiting strain, or a limiting stress, to its modern use as a transgression of any limit to a structure's performance.
- Changes of meaning of the many phrases concerned with ways of incorporating safety into design procedures: proof testing, model testing, factor of safety against collapse, load factor, stress factor, geometrical factor of safety, partial factors of safety, probabilistic views of safety.

This chapter has sought to broaden the discussion of revolutions in engineering design from the particular cases of previous chapters. It has not been the intention to try to 'prove' that such revolutions have actually occurred—such an aim is impossible; rather, it has been to suggest an alternative way of looking at the history of structural engineering, and of design in particular. This approach does, I believe, avoid most of the problems associated with the history of engineering which were identified in Chapter 2 and it integrates a view of history with a number of useful ways of looking at the nature of modern structural design and the role of theory in design. It remains only to suggest a number of conclusions which would seem to be consequent on many of the views put forward in this book.

15

Some concluding remarks

15.1 STRUCTURAL ENGINEERING DESIGN PROCEDURE

In this book I have tried to illuminate some of the problems inherent in seeing the world of engineering as divided into two camps—theory and practice. I have tried to emphasize the importance of design as an activity distinct from the study of the engineering sciences and from the the process of manufacture or construction.† This has been achieved by using the the idea of the 'design procedure' as a way of focusing on what a design engineer does. This has several advantages:

- it strongly distinguishes both the aims and methods of engineering designers from those of engineering scientists; engineering scientists, and their methods, are closer to their 'purer' colleagues in the sciences of materials, physics and chemistry, than they are to design engineers;
- it focuses attention firmly onto the activity of engineering design, which tends otherwise to be overlooked, and provides a separate identifiable 'peg' on which to hang much knowledge and many ideas which to not mix happily with scientists' ideas and knowledge;
- it identifies a philosophical concept which can serve as a parameter in the subject of engineering design in a manner similar to the role of the theory or hypothesis in science;
- it enables design to be recognized as a skill rather than simply a body of knowledge;
- it recognizes the fact that a design engineer's creativity comprises more than the ability to conceive of new structures: a designer must also be able to choose an existing design procedure, or to create a new one which will enable an innovative conception to be realized with confidence;

† I heartily applaude the idea that 'design', in general, should be accorded its own identity as a 'third culture' alongside the arts and the sciences (see [Lewin 1981; Fores & Sorge 1978]).

- it acknowledges the influence of all types of engineering knowledge (not only science and theory) upon the design of works of engineering;
- it elucidates the role which theory plays in the work of a designer, indicating how it might help him to think about the behaviour of structures and to assist in the description and the justification of a proposed design; but theory will not lead directly to designs;
- it helps to loosen the ropes which have tended to bond the activity of design exclusively to theory and science; as a result, two popular notions, the idea of 'scientific design' and the idea that rational design must lead, almost inevitably, to a single solution (the 'one best way'), both evaporate;
- it is equally applicable to all times in history.

By looking at the activity of structural design in terms of the design procedure, we immediately see that the theories of engineering science and the various mathematical techniques of structural analysis are but tools which a designer can use to carry out his main tasks with greater ease or speed or confidence, in the same way that models, pencil and paper, and slide rules or calculators are tools. This approach can be extremely fruitful when looking at ways of helping a structural engineer to work more effectively.

While it may seem obvious to think in terms of a designer with his set of tools, it is, for instance, rather striking that it has apparently been ignored by most programmers of computer software written for use by design engineers. When involved with early stage designs, engineers, as much as architects, use dozens and dozens of sketches backed up with no more than 'back-of-envelope' calculations. Later, in the structural analysis stage, a designer wants to take just one or two of the early schemes and to try out many different small variations on the basic idea to see the final effect on cost, weight, building height and so on. At the detail design stage, the main requirement is for complete lists of the sizes of structural elements and a complete set of calculation sheets to go into the file. In fact, hardly any software exists to aid a designer in carrying out any these tasks. There is also a myth that engineers like numbers; actually, engineers enjoy working out the dozens of three-dimensional coordinates of nodes in a structure, and typing hundreds of numbers into a computer, about as much as an architect would—yet only the software written for architects is always graphically based. In general, hardly any software packages yet get anywhere near to allowing the structural engineer to work in the way he or she would like, in following a design procedure right through from beginning to end. Rather too much software has been written without taking into account how designers actually want to work.

15.2 AESTHETICS AND CRITICISM

Until now I have not returned to the issues raised in Chapter 3 concerning the quality of structural design and the criteria by which it might be judged. Now that we have looked at something of what designers are doing, we can begin to imagine how to tell when they are doing it well. Most of what has been written about aesthetics in structural engineering has been devoted to the visual appearance of structures and, in par-

ticular, to bridges (see, for instance, [ICE 1944; Murray 1981]. It has tended to appeal mainly to the architectural qualities of civil and structural engineering design. Sadly, in the hands of some of the more 'scientific' engineers, aesthetics has been seen as a way of establishing *the* correct way to design a bridge, with the aim, one feels, of getting it written into a Code of Practice. It seems to be particularly difficult for many engineers to get away from the idea that when they design they are following a predetermined path: and if only they could discover the direction of the path (its equation?), aesthetic quality could be guaranteed, just as maximum deflexions can be guaranteed.

Rather little has been written on good design from a more technical point of view. Nervi was one of the very few: he entitled his book *Aesthetics and Technology in Building* and wrote four exemplary short articles under the heading 'Critica delle structure' [Nervi 1959, 1966]. And yet most engineers do have a well-developed sense of excellence in design—they can recognize good and bad ways of solving a problem, an elegant combination of structural elements and the choice of a design solution which succeeds in *avoiding* difficulties rather than surmounting them. Engineering designers do seem more reluctant to talk about what they do and why, than practitioners of other creative arts and there are also far fewer observers and commentators on engineering design than in architecture and music, for example. There would seem to be a general tendency to overlook the highly individual and subjective contribution that a structural engineer can make to the final design of a building which can often be no less creative than the contribution of an architect.

There are, of course, difficulties in criticizing works of engineering: they belong to people who might worry if their building or bridge were judged to be of poor quality.† It is also difficult to know what an engineer's precise intentions were—they may have been strongly influenced by factors outside his control: the architect, the client, costs, time and so on. Yet, although many of these factors also apply to the architecture of a building, there is nevertheless a highly developed subject of architectural criticism. At the very least, it would be a worthwhile goal for engineers to be familiar with the good exemplars of their art, both in the recent and the more distant past, in the same way that architects and artists generally are. Just as with architects and musicians, engineers would greatly improve their powers of analysis and understanding by developing the ability to explain *why* a certain structure is well designed and another less well designed, and to go into technical detail about *why* Eiffel or Maillart or Torroja were great structural engineers. Recently, in fact, a few books have been written which do address these issues, both at a general level [Billington 1985; Holgate 1986] and concerning individual engineers [Billington 1979; Penfold 1980; Pugsley 1976; Skempton 1981]. Perhaps the tide is turning, but there is still a long way to go before criticism and the study of the aesthetics of structures reaches the level of development one finds in the fields of art, architecture, music, literature and cinema.

A developed sense of good design is, however, not merely a matter for the judges making annual awards on behalf of the steel and concrete industries. It should be the concern of every design engineer, otherwise he would have no way of being able to

† Recently one bridge designer told me that when he had informed his employer he intended to publish an article on the aesthetics of bridges using some of his company's bridges as examples of poor design (aesthetic not technical), he was told clearly that he ought to think carefully about his job prospects.

choose which is the better of any two of his trial structural schemes (occasionally one feels that some designers avoid this problem by proposing only one design!). The choice between alternatives touches the work of the designer at another level, namely in selecting or creating a design procedure appropriate to a given structural problem. This type of choice might concern how much to rely on precedent and standard solutions, or how much innovation would be appropriate, or which mathematical model to use, or whether a physical model will improve understanding of the problem and the route to a solution, and so on. A great deal of this type of choice and decision is summed up in the phrase 'engineering judgement', as if it is something which cannot be externalized; and yet, if wine-tasters can develop a vocabulary of aesthetics and quality, surely structural engineers can.

We have seen from looking at the structure of revolutions in structural design that they are very much bound up with feelings about the quality of design procedures. Crisis arises when people come to doubt the effectiveness of a design procedure to deliver safe and economical structures or its power to justify proposed designs. Such doubts are aesthetic judgements under another name. Similarly after a revolution: rival design procedures virtually compete with one another for acceptance and people argue about the relative merits of each—often with difficulty because of the incompatibility of the paradigm concepts of each design community.

Without going into more detail here, it would seem that there could be great benefit in engineers, commentators on engineering and educators opening up more debate about the excellence of, for example, finished designs, design procedures and models used in design and analysis and the criteria upon which such judgements might be based. We have seen the importance of the decisions and choices which design engineers must make in creating or selecting design procedures and scheme designs *appropriate* to the circumstances of a particular project: deciding what is appropriate is a skill which needs to be encouraged and developed just as much as skills of numeracy or using computers.

To encourage the debate, here are just a few criteria by which works of structural engineering might be judged:

- clarity with which structural actions, such as tension, bending and shell action, are used;
- skill with which structural actions are expressed or hidden;
- structural honesty, or dishonesty;
- overall simplicity of structure;
- expression of simplicity or complexity;
- expression of structural elegance;
- economy of material;
- appropriateness of choice of material to structural function;
- expression of the method of construction of the structure;
- ease of construction;
- elegance of joints;
- expression of structural actions in joints;
- juxtaposition of the materials and functions of structural members at joints;
- expression of the size and magnitude of imposed loads in a structure's geometry;

- selection of a structural form which sensibly anticipates the means by which it can be described (defined) and justified;
- expression of an appropriate degree of solidity or delicacy;
- expression of natural geometries such as the circle or catenary;
- degree of integration between load-bearing aspects of a structure and non-load-bearing aspects, such as cladding and building services;
- allusion to structural precedents from history, other cultures or nature;
- expression of a sense of humour or wit.

I hope this list encourages readers to identify their own examples which illustrate the various criteria, and moves them to develop their own (longer) lists. Should two readers then meet, perhaps there will be some debate!

15.3 RESEARCH AND PROGRESS IN STRUCTURAL ENGINEERING

The research and development effort in structural aspects of the construction industry in Britain is carried out mainly in academic establishments and tends to be focused on two main areas—engineering science and materials science. Relatively little work is reported on the development of improved ways of designing structures (from the designer's point of view). The widespread assumption is that it is up to design companies to harvest the fruits of academic research and 'put them into practice'. In many ways, the picture is little different from how it was a century or so ago, as observed in the opening chapters. The activity of design tends to fall between several stools and fails to attract its due attention.

Techniques of design have, nevertheless, progressed and continue to do so, although, as we have seen, not as an automatic consequence of progress in the engineering sciences. They have generally progressed in a relatively ad hoc way as a result of accidents or particular design problems, not as a result of concerted research programmes.

Progress in the state of the art of completed structures depends upon progress in design procedures just as much as in the engineering science and technology which makes up a significant part of the knowledge upon which designers draw when designing structures. The work by the Steel Structures Research Committee and at Frei Otto's Institute of Lightweight Structures were rare and productive exceptions to this rule. The committees which draw up the codes of practice nowadays make perhaps the greatest contribution to establishing what designers should do, and how they should do it, but these come under constant criticism by the design profession. New codes are often felt to be unnecessarily complicated, too dominated by the views of engineering scientists from universities, too restrictive and inflexible, and too little concerned with the real requirements of the designer (see for example [Beal 1979]). Sometimes, the fundamental reasons behind changes to codes is challenged on the grounds that previous codes worked satisfactorily and often resulted in cheaper structures [Beeby 1986].†

† As a reaction to the recent adoption of a new code for the design of reinforced concete structures in Britain (BS 8110), the 'Campaign for Practical Codes of Practice' issued its own, much simpler, non-approved code based on a long-since superseded code, CP 114. In a nationwide survey conducted by the Institution of Structural Engineers, similar ideas gained considerable support [Referendum 1987].

There remains the question as to what sort of research could be carried out into structural design to complement the often valuable research in engineering science. Just as the aims of the engineering designer and engineering scientist are different, so would be the respective aims of their research programmes. If improved design procedures are to be developed, then research needs to be directed specifically towards this end. In the light of many of the subjects discussed in this book, the following might be likely topics for systematic investigation and research:

- the critical appraisal of the results of using different design procedures for the same or similar structures;
- the relative influence of different design criteria and boundary conditions on the weight, safety and cost of the completed structure;
- the effects of different assumptions made in modelling structures, materials and loads;
- investigation into the means by which assumptions made in existing design procedures can be made fully explicit to guard against inadvertent misuse (especially important when computer programs are being used);
- assessment of the power of different methods of justification used in design procedures, e.g. methods of calculation, analytical techniques, empirical data, model testing, proof testing, 'rules of thumb', precedent, intuition;
- investigation of the criteria by which designers choose between different methods of justification, e.g. simplicity, speed, previous efficacy, cost, plausibility, appropriateness, efficiency (i.e. results divided by effort), tradition, directness, range and variety of possible application;
- investigation of the boundaries of application of design procedures, and the circumstances which lead someone to believe that a new case is 'in relevant respects' similar to past cases;
- the search for current dissatisfaction in the design profession, for instance, a loss of faith in current procedures, such as the widely perceived irrationality prior to the plastic design revolution (in Kuhnian terms, the search for anomaly and states of crisis);
- the development of more effective ways in which design procedures can be recorded and hence, judged, improved, communicated, learnt and taught;
- the development of ways in which the skills of engineering design, and the selection and creation of design procedures, can be taught and learnt;
- the development of more effective techniques by which engineering knowledge, in its widest sense, including the understanding of structural behaviour which is so essential to the skill of design, can be taught and learnt.

15.4 THE EDUCATION OF STRUCTURAL ENGINEERS

If the arguments in earlier chapters have been persuasive, then there are important consequences for the education which student and young professional engineers receive. If it be accepted that 'theory' does not lead or direct 'practice', and that engineering design be not merely 'putting theory into practice', then engineering education should incorporate an increased concentration upon design as the central

activity of engineering. It should include design as an identifiably distinct skill which needs to be nurtured and developed separately. A move towards this goal has been frequently suggested (for instance by [Beckmann 1966, Pugsley 1980, Harris 1980, Cowan 1981, Dunican 1981, Lewin 1981, Brohn 1982]). Recently the first professorial chairs of structural design were inaugurated in Britain and more universities are now offering long degree courses which include a major design-related project; but there remains a long way to go. Such large design projects often tend to concentrate on the practical aspects of engineering (manufacture)† or on training students to use the professional Codes of Practice. Neither of these is surely appropriate to an academic education in which it should be intellectual skills which are being developed. Just as it has been possible to develop academic and intellectual approaches to the analytical side of engineering, and to the creative side of architecture, so there needs to be developed an academic and intellectual approach to the creative side of engineering (design) based on rhetoric, repetition and criticism.

At a more particular level, there would be benefit in introducing the notion of the design procedure to students of engineering. This would allow a variety of important issues to be presented and discussed:

- the respective roles of engineering science and other types of engineering knowledge in engineering design;
- the possibility of a proposed design being justified in different ways;
- the relative importances of mathematical analysis, empirical rules, precedent, experience, model tests, and Codes of Practice;
- the many factors which affect the selection of mathemetical models chosen to represent a structure, the materials of which it is made and the loads it must resist;
- the many criteria, such as various notions of failure and economy, which need to be considered when designing;
- the different notions of safety incorporated in different design procedures;
- the criteria by which engineering designs can be compared, both with the problem to be solved (the client's brief), and with one another;
- the critical evaluation of structures;
- the critical evaluation of different types of design procedure;
- the critical evaluation of design engineers.

A direct approach to the understanding of structural behaviour is also to be encouraged, rather than relying on the dubious assumption that such understanding necessarily follows from learning the mathematics of structural analysis [Brohn 1984]. Ultimately, this type of knowledge is the *only* check on the 'validity' of using engineering science and theory in design procedures. An understanding of engineering design and structural behaviour could be further developed by also including a critical study of the history of both structural designs and the design procedures which made them possible (see §15.5).

Finally, much of the above could be incorporated and integrated into the education of engineers while at universities and polytechnics and their training as young profes-

† A knowledge of materials and manufacture is, of course, essential, but would be preferable much earlier in a course, not as part of the climax of an intense academic education.

sionals, by means of methods more usually associated with the learning of arts such as architecture, music and painting (see §15.2). In such fields it is normal for students to undertake work which has a variety of different possible outcomes. It is also usual for a student's work to be discussed and criticized openly rather than marked as simply right or wrong. This is often done in a sort of 'master class' when the work of one student is criticized by a group of teaching staff, practising professionals and other students (the 'crit' or 'jury' in schools of architecture and colleges of design). This practice is virtually unkown in engineering departments. In following this approach, the possibility of variation, subjectivity and choice in engineering could be acknowledged openly. This attitude would contrast strongly with the (usually implicit) belief that engineering 'theory', calculations and analysis lead to unique correct solutions—a belief encouraged by the type of problems engineering students are usually asked to solve in examinations and elsewhere. This step is essential if more engineers are wanted who are able to handle the uncertainties of people and the real world, rather than only the certainties of solutions to mathematical problems.

15.5 THE HISTORY OF STRUCTURAL ENGINEERING

An alternative view of the history of engineering has been presented in this book which makes use of the concept of the design procedure to help to focus historical study on the central activity of the engineering designer. Kuhn's model of historical development has helped to improve our understanding of the histories of both engineering science and design. This has enabled the idea of the 'design revolution' to be developed. Looking back at the problems with conventional approaches to the history of engineering discussed in Chapter 2, the issues have been largely resolved:

- it is not necessary to divide the history of structural engineering into two ages, before and after the supposed first use of theory;
- attention is focused on engineering design which encourages a direct assault on the principal historical question which intrigues many modern engineers: 'how did engineers in the past design their artefacts?';
- the history of engineering science can be treated separately from the history of design, and in ways similar to other sciences such as astronomy, physics and chemistry;
- the intellectual nature of design is emphasized; this highlights the importance of the development of the language and concepts used by designers, which are often masked by the changing usage and meaning of words and by the tendency to describe the past in terms of modern ways of thinking.

There is, I believe, a strong argument that similar approaches could be taken in the histories of other branches of engineering design. Some excellent work on the history of the theory and practice of water wheels would, perhaps have been clarified by looking at the histories of the relevant science and design procedures [Reynolds 1973, 1979]. Two authors have mentioned the idea of design revolutions in treating the histories of steam engines [Duffy 1979] and the jet engine [Constant 1980]. Both, however, have concentrated mainly on those aspects of progress which have resulted from inventions, rather than on developments in intellectual approaches to both thermo-

dynamic science and to heat engine design. The story of 'caloric' and its replacement by the idea that heat is a form of energy, would seem to follow the classic pattern of a Kuhnian revolution. It would also seem highly likely that one could find evidence of scientific and design revolutions in the history of mechanical engineering which are of a different type to the undoubtedly important changes resulting from mechanical inventions. Returning nearer to our present subject, the most rewarding area for a historical study of the kind advocated in this book, would be the history of soil mechanics and the related 'practical' subjects of foundation and dam design: as well as a long history of alternative theories and design methods, there are, at present, clear examples of quite distinct scientific and design communities.

Finally, it is worth underscoring some of the many benefits which could arise from the study of the histories of engineering design and design procedures, particularly at a time when its place in university education is falling from 'very little' to even less. There are, first of all, several very direct benefits to be gained (see [Hamilton 1944/45; Mainstone 1977]). The main reason for this is that there are, as we have seen, two types of knowledge in engineering: one type can conveniently be communicated to others in textbooks, equations, tables, drawings and so on; the other type can only be learnt by experience, direct experience of structures which have been built and can be seen to work, and behave in certain ways. The study of precedent is arguably the richest source of structural knowledge and understanding.

It is, therefore, of immeasurable value to build up a vocabulary of types and sizes of structures which have been constructed of different materials—masonry, cast and wrought iron, timber, steel and reinforced concrete, and the types of sections and joints which are characteristic of these materials. Part of the value of this can be to help with modern design—there is little point in re-inventing the wheel and there are now relatively few (maybe, no) genuinely new ideas for solving structural problems. An obvious advantage of studying which types of structure have survived is that poor designs have usually not stood the tests of time. Over millions of years, this same principle of natural selection has preserved the very best practice in structures in the natural world around us: these can also be a source of inspiration for structural ideas.

The study of old structures has a particular advantage. It is often difficult to see how many modern structures work, since crucial members and joints are often covered up behind internal and external cladding, building services or furniture, or within the mass of reinforced concrete. Old structures are often more transparent to read and can give unmistakable clues to their structural behaviour in the form of observable deformations and cracks. They also often exemplify the more basic forms of structural action—compression structures and trusses, rather than the more complex actions of portal frames and prestressed structures. Of perhaps more immediate use, a knowledge of historical structures can also be of help in understanding the construction of old structures which are in need of restoration.

More generally, it can be challenging to try to work out how an unfamiliar structure is working, and, rather more difficult, how its designer might have thought it was meant to work. It is always instructive and sometimes surprising, gradually to uncover more and more examples of elegant design, from the overall scheme down to minute details, in a structure by great engineers such as Brunelleschi, Wren, Telford, Schwedler, Roebling, Hennebique, Maillart, Freyssinet, Nervi, Torroja, Arup and other 'structural artists', as they have recently been christened [Billington

1985]. It is most important for us to resist the common temptation to believe that, because of our wonderful new materials, construction plant and computers, everything we do nowadays is better than in previous ages before all our modern progress. A relatively cursory look at history will indicate that one thing which has not improved is man's capacity for ingenuity and consummate skill. Giving structures, old and new, this sort of detailed consideration is a very valuable way of developing the skills to recognize a structure as an example of excellent design, to criticize structures and to practise rhetoric. It can give more experience of the design and behaviour of structures in a few hours than many months spent working at a computer or on a current live project.

At the most general level, we must recall that engineering design is an art which demands a considerable knowledge of the real world and its behaviour, as well as many branches of mathematics and engineering science and skills such as computing. Even assuming that theoretical matters can be learnt from lectures, textbooks and solving mathematics problems, they are as seeds sown on desert sand without *some* experience of relevant aspects of the real world. It is quite astonishing how many graduates arrive in civil and structural engineering design companies without even the vaguest idea of even the current types of dam, bridge, roof and building which are being designed and constructed, let alone the almost infinite variety of such works in the past. They have little or no concept of how and why current designs have evolved to their present stage. They have virtually no knowledge of the masterpieces of 3000 years of civil and architectural engineering which still exist and can be visited, let alone the many more which have now gone. They have even less idea of *why* they were masterpieces. They would be hard pressed to name ten 'famous' engineers from the whole of history, let alone any of the thousands of equally interesting people who, for various reasons, are not now classed as famous. A young musician, architect or painter who had a similar knowledge of their own cultural background would, quite rightly, be ashamed. It is perhaps hardly any wonder that engineers, as a profession, have a rather low self-esteem and a poorly developed sense of professional identity and public image, compared, for instance, to architects, doctors, musicians and lawyers.

15.6 POSTSCRIPT

Some readers might have expected these conclusions to include some clear instructions as to how a designer could become a better designer. This has not, however, been the explicit purpose of this book. Rather, the aim has been to present an alternative way of looking at design which could awaken in its practitioners a greater degree of self-awareness. This might make some difference, but then again . . . . It can, however, be argued that now is a particularly important time to raise many of the issues I have discussed.

The senior engineering designers of today still belong to an era in which they developed their skills and structural attitudes 'manually'. The difficulty and tedium of complex calculations forced them continually to seek ways of simplifying their task and to have frequent recourse to their qualitative knowledge of the behaviour of structures by way of checking calculations which they knew to be approximate. Some

design procedures were particularly preferred because they approached design in terms which closely paralleled the designer's qualitative understanding of structural behaviour. Graphical statics and the moment distribution method (developed by Hardy Cross) are perhaps the best known of these—they both encouraged designers to think about structural behaviour almost as if they themselves were the structures having to resist the applied loads.

The younger generation of designer engineers has not developed its understanding of structural behaviour in this way. It is encouraged more and more to concentrate upon the 'behaviour' of computer-based mathematical models of structures which are believed to generate ever more accurate results. The fallibility of such results is seldom acknowledged. The understanding, knowledge and assumptions upon which these methods are based tend to be overlooked or ignored and are likely soon to be forgotten. The faith in the results is tending to become blind faith.

This situation has a certain resemblance to that which existed concerning the design procedures for Gothic cathedrals in the case of the partly complete cathedral in Milan, around the year 1400, and again in Beauvais 170 years later, when the 153-metre tower, added to the existing cathedral, collapsed just four years after its completion:

> The decay [in understanding] sensed by the eye after about 1250 stems from a slow relaxation of the firm structural grasp that had been acquired during the preceding hundred years. Certainly, one hundred years later, by the end of the fourteenth century, the Milan expertises demonstrated that the Italians, at least, did not know how to build a cathedral . . . [Design] rules were, to Mignot [a French designer who presented evidence at Milan] something out of a book, which had been tested by time and practice, but whose meanings and, indeed, whose very reasons for existence, were becoming more and more dim with the passage of time . . . .
>
> There was at that time what might be called a European Code of Practice for the design of cathedrals; the experts assembled at Milan had come from all over Europe, and were in broad agreement on the codes they had in their pockets. But in Milan in 1399, and again in Beauvais in 1569, [the tower of which was, . . . as far as is known, designed in accordance with the best rules of the time . . . , the reasons behind the rules were no longer understood, and it is always dangerous to design blindly from a code of practice. [Heyman 1967/68:15; Sibly & Walker (discussion) 1978:323]

There are many anecdotes similar to that of the young designer firmly believing the output of a structural analysis computer program, despite the obvious impossibility of a building deflecting into the wind; and of another who detailed a two-storey frame building with genuine pin-joints because that was how he had analysed the structure; and of a third who believed the computer's output showing the sum of foundation reactions greater than the total weight of the structure and imposed load. Two of these errors were detected before construction commenced; fortunately for the designer of the 'pin-jointed portal frame', there wasn't much wind before a senior engineer happened to arrive on site and notice the error. However, the danger of similar errors remaining undetected seems likely to grow with the use of computers, and the occurrence of a serious collapse seems only to be a matter of time. A recent publication sensibly advocates checking the output of computer software by comparing the results with the engineer's qualitative appraisal of the problem and an approximate analysis performed by hand [MacLeod 1988]. However, I have also heard it

said on many occasions that the way of checking computer results is to compare them with those generated by a different piece of software (what do you do when they give different results?). I once saw a computer beside which was a glass-sided box carrying the label, 'In case of emergency, break glass'. It contained an abacus. Adjacent to the structural engineer's computer, the glass box should, perhaps, contain a small Meccano set and a couple of plastic rulers.

APPENDIX 1

Vitruvius and the origins of the theory/practice classification

A1.1 INTRODUCTION

By way of an aside to the main argument of Chapters 1 and 2, concerning the application of the theory/practice classification to civil and structural engineering, it is interesting to look at one of the very earliest texts in the subject—the text which, both directly and indirectly, has influenced virtually all designers of buildings for the last 2000 years—the *Ten Books on Architecture* by Vitruvius.

Vitruvius wrote his book in around 25 BC in the midst of one of the major rebuildings of Rome, basing it on many Greek books which are now lost to us. The title, as translated into modern languages, is rather contentious for 'architecture' and the job of the 'architect' were not then what is now implied by the words. In many ways, the contents of the book would suggest a title referring to architecture, civil and military engineering. The Greek word *'architecton'*, from which 'architect' derives, incorporates the idea of organizing people to achieve a goal, so perhaps the phrase 'construction management' should also be included.

Several Latin manuscripts of the work survived from the time of its writing. After the Gothic design revolution many tens of manuscript copies were made and the book became widely known again. The first printed version appeared in Italy in 1486, making it one of the earliest books printed using moveable type. By this time Alberti had already drawn heavily upon its contents for his own *Ten Books on Architecture* published posthumously in Florence in 1485. Alberti's book was the first of many Renaissance treatises which broadcast the Classical approach to the design of buildings throughout Europe.

Although both books were already well known in England, the first translation of Alberti into English was in 1755. Vitruvius was translated into English, first, in 1692, via Perrault's translation into French and later, in 1791, from the original Latin. Since then there have been three more translations, in 1826, 1914 and 1931.

The 140-year period covering the major English translations spans just that era during which the attitude of designers to 'theory' was changing. This was happening in parallel with the developments of mathematics (including statics), the experimental sciences and the change away from the traditional materials for building, towards cast and wrought iron and, later, steel and reinforced concrete.

The translations given below of the opening paragraphs concern the nature of building design and what a designer (architect) does. They are illuminating in a number of ways:

- they illustrate the early use of the terms 'theory' and 'practice' in both French and English;
- they illustrate the serious doubts which early translators (two architects and an engineer) felt about appropriateness of these words to express the meaning conveyed in Latin (Perrault (1684), Newton (1791) and Rondelet (181277;
- they illustrate the contemporary attitudes of the translators to the job of the architect or engineer;
- they illustrate the difference between translations by classics scholars and that given by Rondelet, an engineer;
- they give examples of the frequent exhortations to combine 'theory' and 'practice' long before 'theory' in the modern sense of the word, existed;
- they provide an interesting comparison with the ideas of *'geometria theorica'* and *'geometria practica'* which were current in 12th and 13th centuries treatises on geometry (see §12.3).

A1.2 THE LATIN ORIGINAL

1.1.1
Architecti est scientia pluribus disciplinis et variis eruditionibus ornata [cuius indicio probantur omnia] quae ab ceteris artibus perficiuntur. Opera ea nascitur ex fabrica et ratiocinatione. Fabrica est continuata ac trita usus meditatio, quae manibus perficitur e materia cuiuscumque generis opus est ad propositum deformationis. Ratiocinatio autem est, quae res fabricatas sollertiae ac rationis proportione demonstrare atque explicare potest.

1.1.2
Itaque architecti, qui sine litteris contenderant, ut manibus essent exercitati, non potuerunt efficere, ut haberent pro labris auctoritatem; qui autem ratiocinationibus et litteris solis confisi fuerunt, umbram non rem persecuti videntur. At qui utrumque perdidicerunt, uti omnibus armis ornati citius cum auctoritate, quod fuit propositum, sunt adsecuti.

1.1.3
Cum in omnibus enim rebus, tum maxime etiam in architectura haec duo insunt, quod significatur et quod significat. Significatur proposita res, de qua dicitur; hanc autem significat demonstratio rationibus doctrinarum explicata.

[Vitruvius (ed. Granger) 1931]

A1.3 THE 1684 FRENCH VERSION

1.1.1
L'Architecture est une science qui doit estre accompagnée d'une grande diversité d'estudes & de connoissances par le moyen desquelles elle juge de tous les ouvrages des autres arts [qui luy appartiennent]. Cette science s'acquiert par la *Pratique,* & par la *Theorie:*† la Pratique consiste dans une application continuelle à l'execution des desseins que l'on s'est proposé, suivant lequels la forme convenable est donnée à la maniere dont toutes sortes d'ouvrages se sont. La Theorie explique & demontre la convenance des proportions qui doivent avoir les choses que l'on veut fabriquer.

1.1.2
Cela fait que les Architectes qui ont essayé de parvenir à la perfection de leur art par le seul exercice de la main, ne s'y sont gueres avancez, quelque grand qu'ait esté leur travail, non plus que ceux qui ont cru la seule connoissance des lettres et la seule raisonnement les y pouvoit conduire; car ils s'en ont jamais vu que l'ombre: mais ceux qui ont joint la Pratique à la Theorie, ont esté les seuls qui on reüissi dans leur entreprise, comme s'etant munis de tout ce qui est necessaire pour en venir à bout.

1.1.3
Dans l'Architecture comme en toute autre science on remarque deux choses; celle qui est signifiée, & celle qui signifie:‡ la chose signifiée est celle dont l'on traite, & celle qui signifie est la demonstration que l'on en donne par la raisonnement soustenu de la science.

† Les mots de *Fabrica* & de *Ratiocinatio* de la maniere que Vitruve les explique, ne pouvoient estre autrement traduit que par *Pratique* & *Theorie*, parce que *raisonnement* est un mot trop general, et que *Fabrique* n'est pas François [français].

‡ Je croy que Vitruve entend par la chose signifiée celle qui est considerée absolument & simplement telle qu'elle paroist estre, & par la chose qui signifie, celle qui fait que l'on connoist la nature interne d'une chose par ses propres causes. Ainsi dans l'Architecture un Edifice qui paroist bien basty est la chose signifiée; & les raisons qui font que cet Edifice est bien basti, sont la chose qui signifie, c'est a dire qui fait connoistre quel est le merite de l'ouvrage.

[Vitruvius (trans. Perrault) 1684]

A1.4 THE 1692 (ABRIDGED) VERSION

1.1.1
Architecture is a Science which ought to be accompanied with the Knowledge of a great many other Arts and Sciences, by which means it forms a correct Judgement of all the Works of other Arts that appertain to it. This Science is acquired by Theory and Practice. The Theory of Architecture is that knowledge of this Art which is acquired by study, travelling and discourse. The Practick is that knowledge that is acquired by the Actual Building of great Fabricks.

1.1.2
[Omitted]

1.1.3
These Two Parts are so necessary that never any came to any great Perfection without them both. The one being lame and imperfect without the other, so they must walk hand in hand

[Vitruvius (trans. from Perrault and abridged) 1692]

A1.5 THE 1791 VERSION

1.1.1
Architecture is an art comprehending many sciences and various kinds of erudition; by the rules of which, the works of all other arts are examined. It consists of practice and theory.† Practice is the constant and accustomed attention to the manual operations, and to the several kinds of materials of which a work may be constructed. Theory is the ability to demonstrate and explain the rules and reasons of the proportions of buildings.

1.1.2
Architects who have practised without theory, and who have been only experienced in the manual part, have not been able to acquire any reputation by their works; and those who have trusted to theory and speculation only, have followed the shadow and not the substance; but those, who are perfectly acquainted with both, like men completely armed, speedily, and with reputation, succeed in their endeavours.

1.1.3
For as in all things, so especially in architecture, there are two parts, the signified and the signifier; the former is that which is here proposed to be treated of; the latter is the demonstration of the principles of the sciences explained.

† The words of the text are 'Fabrica' and 'Ratiocinatione', which most of the translators have agreed in rendering practice and theory: but the definitions of Vitruvius, give us to understand, that by practice, he does not mean the actual labour of the workmanship, but the knowledge thereof only, so as to be able to direct the workmen and to know when the work is well performed, and the materials good; as to theory he means the knowledge of the proportions, forms, distributions etc. of the several parts of buildings and their effects, so as to be able to design or compose with judgement.

[Vitruvius (trans. Newton) 1791]

A1.6 THE 1812 VERSION

1.1.1
L'architecture est une science qui comprend plusieurs préceptes et diverses connaissances, au moyen desquels elle peut apprécier les ouvrages des autres arts qu'elle dirige; cette science est le résultat de la pratique et de la théorie. La pratique est l'objet des opérations manuelles nécessaires pour donner à la matière la form qu'elle doit avoir, pour quelques genres d'ouvrage que ce soit. La théorie est la science qui peut expliquer et démontrer les procédés et la justesse des proportions des ouvrages éxecutés.

1.1.2
C'est pourquoi les architectes qui, sans instruction, ont voulu suivre cette carrière, ne sont jamais parvenus, quelque exercés qu'ils fussent dans la pratique des arts, à faire des ouvrages qui puissent etre cités pour exemple. Mais aussi ceux qui ne se sont occupé que de raisonnement abstraits et de littérature, paraissent avoir plutot suivi l'ombre que l'objet. Quant à ceux qui se sont également appliqués à la théorie et à la pratique, ayant toutes les connaissance nécessaires, ils sont parvenus à faire des ouvrages dignes de servir de modèle.

(Translation of Vitruvius given in [Rondelet 1812–17:book 5])

Rondelet continues by noting:

Tous les auteurs qui, depuis Vitruve, ont parlé de la théorie et de la pratique, les ont considérées indépendamment l'une de l'autre. Les uns pour faire valoir la théorie, se sont plu à présenter la pratique comme une routine aveugle qui ne fait les choses que par imitation, sans raisonnement ni principes. Les autres, par opposition, ne trouvent dans la théorie que des raisonnements abstraits, dont l'application n'est pas d'un grande utilité dans les arts. Mais ces deux extremes n'existent pas.

Le mot théorie vient du grec *'theoria'*, que Vitruve traduit en latin par ratiocinatio, et qu'il définit en disant que c'est la science qui explique et qui démontre les opérations des arts. Ce mot peut etre traduit en français par raisonnement. Cependant, on pourrait plutot dire que raisonnement est le moyen dont se sert la théorie pour faire connaitre le résultat de ses observations; car la vraie signification de *'theoria'* est contemplation et méditation profonde.

Ainsi le premier objet de la théorie doit etre l'observation; en effet, pour pouvoir raisonner juste sur une matière quelconque et en bien juger, il faut avant tout la bien connaitre. Mais cette connaissance dépend de beaucoup d'autres qu'il est difficile de réunir.

D'abord, il faut examiner le motif qui fait entreprendre un édifice, les dispositions qu'on doit avoir pour remplir sa destination, les matériaux qu'on doit y employer, les formes et les dimensions de chacune de ses parties, tant pour l'usage que pour la solidité, relativement à la charge ou aux efforts qu'elles peuvent avoir à soutenir.

La difficulté de réunir toutes les connaissances nécessaires pour bien juger d'un édifice nous a fait imaginer de considérer la théorie sous plusieurs rapports principaux, qui peuvent etre considérés séparément. D'après cette idée, nous pensons que ce serait à ceux qui font batir, ou à ceux pour qui l'édifice est destiné, à juger si le projet quon leur présente remplit le but qu'ils se sont proposé. . . .

L'objet de cette partie essentielle de l'art de batir est d'examiner les parties d'un édifice relativement à la solidité; d'examiner les moyens d'exécution et d'economie, en ayant égard à l'espèce des matériaux, à leur nature, leur propiété et de la manière dont ils sont mis en oeuvre. Cet examen se fait par le moyen du calcul, de la géométrie et des principes de méchanique. Cependant ces operations ne constituent pas seules la théorie.

[Rondelet 1812–17:book 5, pp2–5]

A1.7 THE 1826 VERSION

1.1.1
Architecture is a science arising out of many other sciences, and adorned with much and varied learning; by the help of which a judgement is formed of those works which are the results of other arts. Practice and theory are its parents. Practice is the frequent and continued contemplation of the mode of executing any given work, or of the mere operation of the hands, for the conversion of the material in the best and readiest way. Theory is the result of that reasoning which demonstrates and explains that the material wrought has been so converted as to answer the end proposed.

1.1.2
Wherefore the mere practical architect is not able to assign sufficient reasons for the forms he adopts; and the theoretic architect also fails, grasping the shadow instead of the substance. He who is theoretic as well as practical, is therefore doubly armed; able not only to prove the propriety of his design, but equally so to carry it into execution.

1.1.3
In architecture, as in other arts, two considerations must be constantly kept in view; namely, the intention and the matter used to express that intention; but the intention is founded on a conviction that the matter wrought will fully suit the purpose.

[Vitruvius (trans. Gwilt) 1826]

A1.8 THE 1914 VERSION

1.1.1
The architect should be equipped with knowledge of many branches of study and varied kinds of learning, for it is by his judgement that all work done by the other arts is put to test. This knowledge is the child of practice and theory. Practice is the continuous and regular exercise of employment where manual work is done with any necessary material according to the design of a drawing. Theory, on the other hand, is the ability to demonstrate and explain the productions of dexterity on the principles of proportion.

1.1.2
It follows, therefore, that architects who aimed at acquiring manual skill without scholarship have never been able to reach a position of authority to correspond to their pains, while those who relied only upon theories and scholarship were obviously hunting the shadow, not the substance. But those who have a thorough knowledge of both, like men armed at all points, have the sooner attained their object and carried authority with them.

1.1.3
In all matters, but particularly in architecture, there are these two points:- the thing signified, and that which gives it its significance. That which is signified is the subject of which we may be speaking: and that which gives significance is a demonstration on scientific principles.

[Vitruvius (trans. Morgan) 1914]

A1.9 THE 1931 VERSION

1.1.1
The training of the architect depends upon many disciplines and various apprenticeships which are carried out in other arts. His personal service consists in craftsmanship and technology. Craftsmanship is continued and familiar practice, which is carried out by the hands in such material as is necessary for the purpose of a design. Technology sets forth and explains things wrought in accordance with technical skill and method.

1.1.2
So architects who without culture aim at manual skill cannot gain a prestige corresponding to their labours, while those who trust to theory and literature obviously follow a shadow and not reality. But those who have mastered both, like men equipped in full armour, soon acquire influence and attain their purpose.

1.1.3
Both in general and especially in architecture are there two things found; that which signifies and that which is signified. That which is signified is the thing proposed about which we speak; that which signifies is the demonstration unfolded in systems of percepts.

[Vitruvius (trans. Granger) 1931]

A1.10 A MODERN VERSION

In the light of the discussions in this book, the following translation of the first paragraph is offered—not literal but, I believe, expressive of the attitudes and intuitions of a building designer (and no less fanciful than some of the other translations):

1.1.1
Architectural engineering is a body of knowledge comprising many disciplines and sciences which can also be applied to other fields of engineering. Finished works are born of construction and design.

The skill of construction comes from the constant study of craftsmanship and the working of materials to create the desired results.

Designing is the ability to describe and to justify the design of buildings by means of the rational use of engineering knowledge and scientific principles.

[Vitruvius (trans. Addis)]

APPENDIX 2

The nature of scientific theories

A2.1 SCIENTIFIC METHOD

The words ‘theory’ and ‘science’ have been used and discussed in many places in the main part of the book, especially the changes in their meaning and use. Until the early 19th century both terms referred to the idea of a ‘body of knowledge’. In the middle of the century they both became specifically associated with natural philosophy and a methodology for the growth of knowledge. This methodology incorporated specifically designed experimentation, inductive logic and the testing of scientific hypotheses (or ‘theories’ as they came to be known) [Poincaré 1914].

It has recently been argued, principally by Popper [Popper 1959], that the inductive model for the growth of scientific knowledge should be replaced by a hypothetico-deductive model. The phrase ‘scientific method’ has come to be associated particularly with this second model. The two models have been summarized as follows:

Inductive view—
(i) observation and experiment
(ii) inductive generalisation
(iii) hypothesis
(iv) attempted verification of hypothesis
(v) proof or disproof
(vi) knowledge

Popper’s view—
(i) problem (usually rebuff to existing theory)
(ii) proposed solution, in the form of a new theory
(iii) deduction of testable propositions from the theory
(iv) attempted refutation by experiment and observation
(v) preference established between competing theories
[Magee 1973:56]

Popper's thesis is that genuine knowledge can only arise from a logically infallible methodology and that this can only be achieved by means of the notion of 'falsifiability', replacing the 'verifiability' of the (fallible) inductive methodology (see [Magee 1973:35ff]). More recently, Popper's views have been challenged by Kuhn. Kuhn's main argument is that there can be no neutral algorithm to establish the preference between rival theories (see (v) in Popper's view, above). Kuhn's views are presented in his *The Structure of Scientific Revolutions* [Kuhn 1970a] and in Chapter 9 of this book. The debate between Kuhn and Popper can be followed up in [Lakatos & Musgrave 1970] and [Kuhn 1977].

A2.2 THE NATURE OF SCIENTIFIC THEORIES

It is important to be aware of all the functions of scientific theories. A follower of Popper might suppose that science is made up of devising a series of dramatic hypothesis-testing experiments. Much of science is rather more mundane, simply collecting more and more evidence in order to fill out every unexplored corner of a branch of science. The summary given below is based upon the introductory lecture to a symposium on 'Logic, Methodology and Philosophy of Science' [Mehlberg 1962]. It provides a useful summary of the functions of scientific theories which will help to illuminate sections of the book, especially Chapter 6 which considered the nature of engineering knowledge. Mehlberg argues that a scientific theory comprises both theoretical and empirical aspects and that the former can be defined in terms of the latter.

A2.2.1 Empirical aspects

Mehlberg suggests that the empirical aspects of scientific theories are determined by five essential functions which must be discharged by a theory:

(i) Informational
(ii) Summarizing
(iii) Predictive
(iv) Explanatory
(v) Controlling

The *informational* function of a theory is perhaps the most fundamental one:

> [Theories] provide us with socially relevant and dependable information about objects which are observable to man, on the understanding that the dependability of this information is due to its being backed up by the outcome of other observations carried out by human investigators. In other words, scientific theories provide us with knowledge. [Mehlberg 1962:278]

A theory is the means by which certain knowledge of the world can be stored. A measure of the success of a theory is thus the storage capacity it has and the ease with which the information can be retrieved.

Following upon the storage analogy, it is the *summarizing* function of a theory to contain its information in the most economic way:

> In the case of mechanics, for example, one of its essential functions is summarizing an infinite number of mechanical laws and thereby making an infinite set of laws manageable. The conciseness achieved by the physicist in this respect is unparalleled. The whole theory of mechanics can be condensed into a single variational principle expressed in a few letters. [Mehlberg 1962:276]

The German physicist, Ernst Mach, went so far as to suggest that the summarizing function was the principal aim of science:

> [Science] may be regarded as a minimal problem, consisting of the completest possible presentment of facts with the least possible expenditure of thought. The function of science, as we take it, is to replace experience. [Mach 1960:586]

The *predictive* function of a theory concerns itself with what has not yet happened:

> the theory is also expected to predict any mechanical law which could possibly come to be discovered; by the same token, the theory must also predict any mechanical fact which would be observed under any specifiable circumstances . . . The predictive success of mechanics is even more spectacular than is summarizing success. [Mehlberg 1962:277]

The *explanatory* function of a theory is, for the scientist, perhaps the most important:

> The essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as matter of course, so that our curiosity rests. 'Reducing a situation to elements' means, from the operational point of view, discovering familiar correlations between the phenomena of which the situation is composed. [Thus] we see that an explanation is not an absolute sort of thing [and] what is satisfactory for one man will not be for another . . . .
>
> If we are explaining the action of a machine, we are satisfied to reduce the action to the push and pull of the various members of the machine, it being accepted as an ultimate that these members transmit pushes or pulls. But the physicist who has extended his experimental knowledge further, may want to explain how the members transmit pushes or pulls in terms of the action on each other of the electrons in the orbits in the atoms. The character of our explanatory structure will depend on the character of our experimental knowledge and will change as this changes . . . As we extend experimental knowledge and push our explanations further and further, we see that the explanatory sequence may be terminated in several possible ways. [Bridgman 1928:37–40]

> As soon as we inquire into the reasons for . . . phenomena, we enter the domain of theory, which, on the basis of hypothesis admitting of more or less direct test, connects the observed phenomena and traces them back to single 'pure' phenomena, thus bringing about a logical arrangement of an enormous amount of observational material. [Joos 1934:1]

Mehlberg also argues that there is a *controlling* function of a theory by which it:

> should enable us to bring about desirable changes in our environment by following procedures which the theory indicates. [Mehlberg 1962:277]

Although widely held, this view rather contradicts some of the views presented in this book. It is a manifestation of the idea of 'applied science' whereby 'theory is put

into practice' and is argued against in Chapter 1 and elsewhere (for example [Layton 1971; Langrish 1974]).

A2.2.2 Theoretical Aspects

In order that the empirical functions of theories may be discharged there must be a 'mechanism' by which the knowledge is put into storage in the form of a theory and may be retrieved at will. This is the role of the theoretical aspects of theories—the means by which the theory is expressed or *formalized*. Mehlberg identifies three distinct aspects:

(i) logical formalism
(ii) mathematical formalism
(iii) metaphysical formalism

The *logical formalism* is the means by which the 'elements' of a branch of science may be built one upon another to generate new knowledge—the reverse of the process of explanation. In logical terms, the 'elements' or axioms and definitions are manipulated according to certain rules—rules of inference, to generate theorems of the axiomatic system. The example of geometry is typical (see [Hospers 1967:193–198]). The most important property of an axiomatic system is that it is necessarily true, given the axioms, definitions and rules of inference, and not susceptible of experimental proof. Thus experiments purporting to prove the 'laws' of statics are doing no such thing. All they do is test the validity of the interpretation of the axiomatic system (see metaphysical formalism, below). Finally. theories need to be logical in order to make them amenable to mathematical treatment.

The *mathematical formalism* of a theory is necessary to express the logical formalism in a way by which the empirical functions of the theory can be discharged. This allows the storage of experimental data and the output of this knowledge and of predictive information in a quantitative form which can then be manipulated according to the rules of the logical formalism. A single logical formalism can be represented in different mathematical ways: for example, vectors can be represented geometrically, trigonometrically, using complex numbers, in vector calculus notation or as matrices.

A *metaphysical formalism* is necessary to give a physical interpretation to the logical and mathematical formalisms. This point can be appreciated by considering the use of mathematical and analogue models of physical phenomena. The process of creating a mathematical model, for instance a pin-jointed model of a truss bridge, is the reverse of the process of interpreting a mathematical formalism. The success of analogue models is based upon the different physical interpretations of one mathematical formalism. An example from structural science is the modelling of the elastic and plastic moduli of beam sections, respectively by the shape of a membrane being inflated through a hole the shape of the section, and by the shape of a heap of sand resting on a platform in the shape of the section [Cowan *et al.* 1968:25ff]. Electrical analogues of structural and hydrodynamic phenomena are also common [Timoshenko 1953:397].

APPENDIX 3

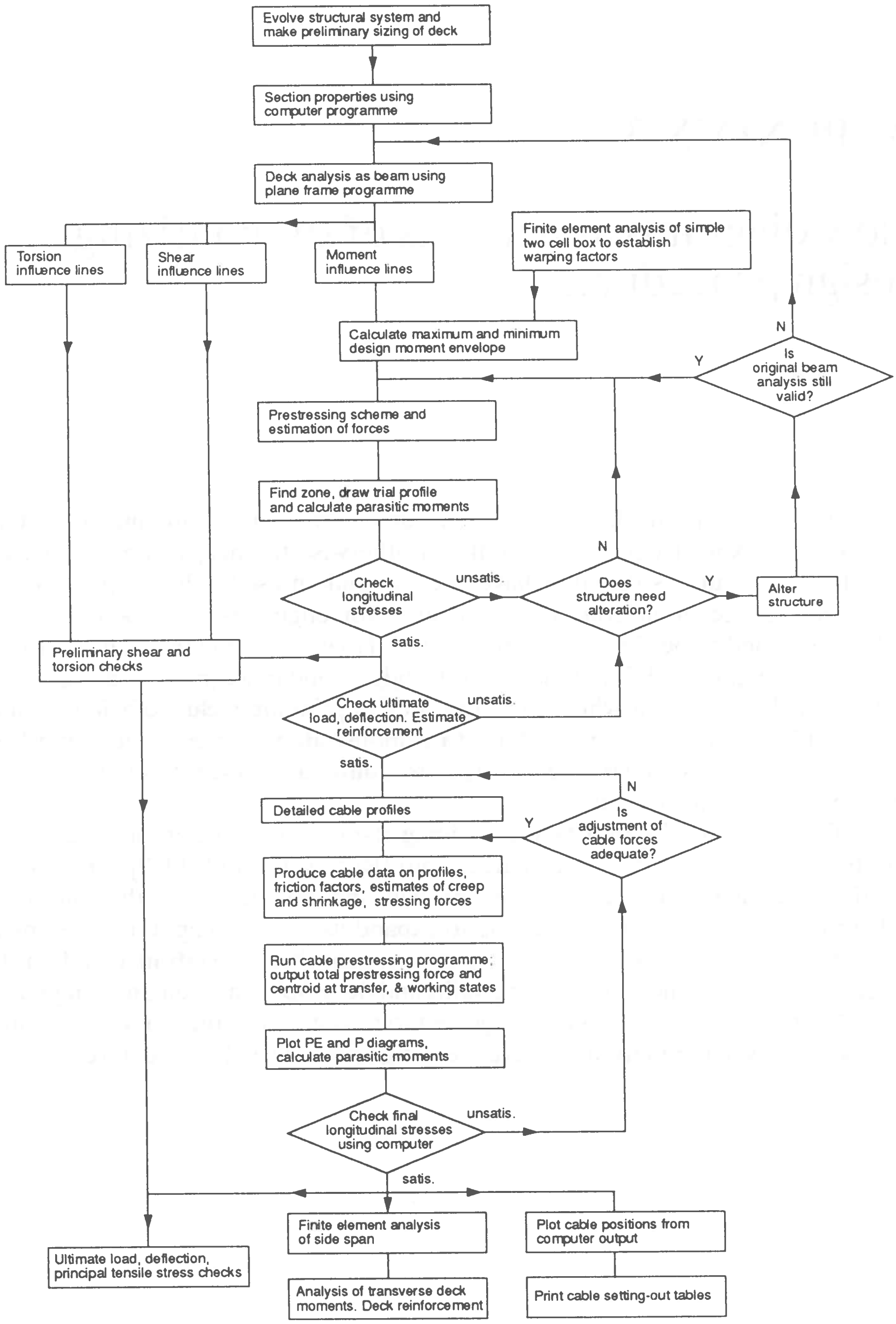
Flow diagrams as a means of summarizing design procedures

Flow diagrams are particularly effective at summarizing and communicating a type of engineering knowledge which is difficult otherwise to encapsulate—the design procedure. It is curious that they have been so seldom used either in professional Codes of Practice or in educational material for engineers. Such knowledge is perhaps assumed to be simply 'common sense'. However, while this may be true of experienced engineers, it is not the case for students and newcomers to design.

Two rare flow diagrams which have appeared in print are included below to illustrate the idea of the design procedure, to demonstrate the effectiveness of a flow diagram as a way of summarizing a design procedure, and to serve as models which others may choose to emulate.

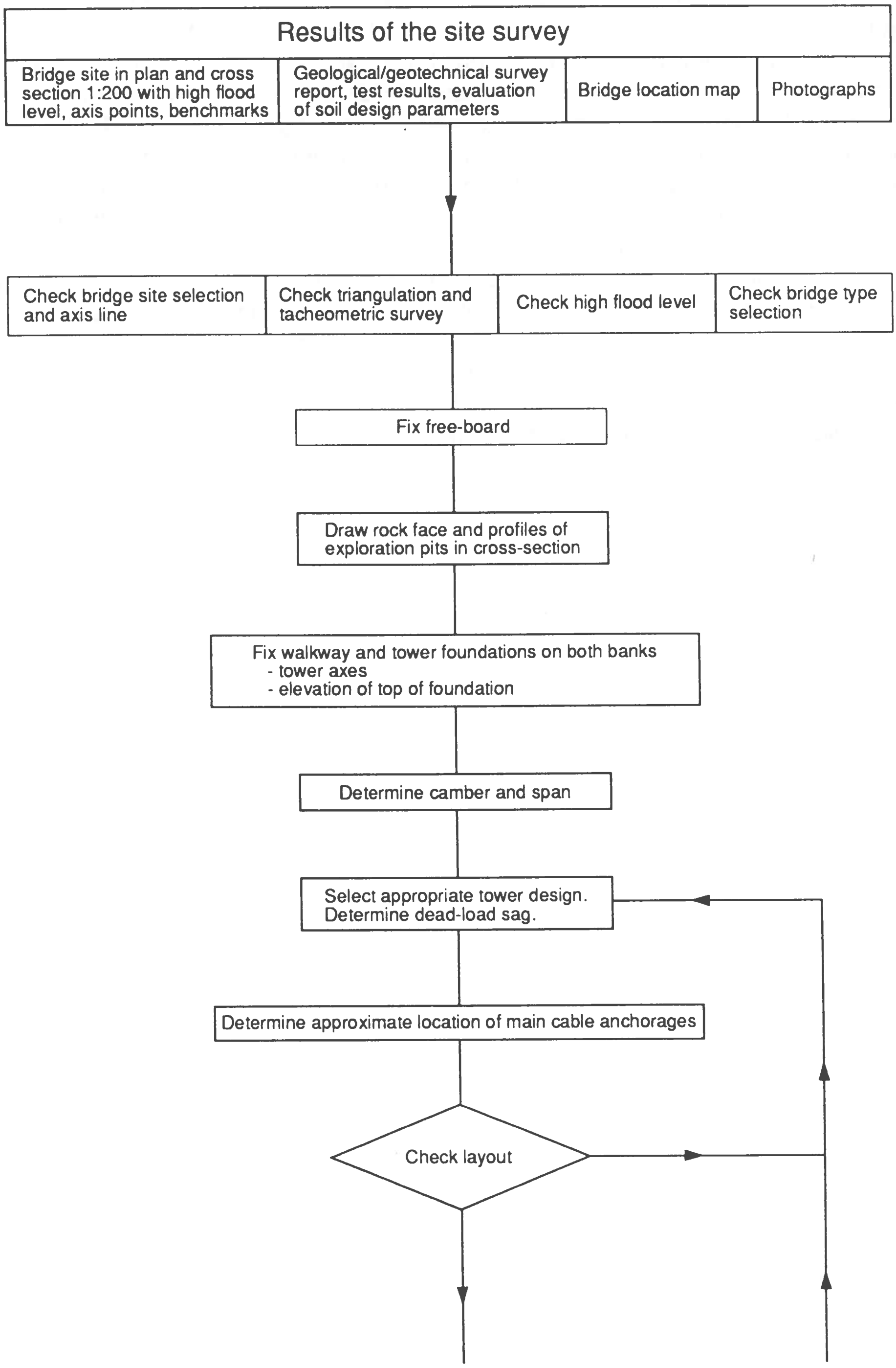
The first appeared in an article describing the design of a reinforced concrete road bridge within the Ove Arup Partnership [Ranawake *et al.* 1970]. The project was the first such structure the company had undertaken and at the time (late 1960s) the company was also beginning to expand its use of computer programs to help with the structural analysis. Both factors meant that the staff involved on the project were learning new engineering skills and developing new engineering knowledge. In order to record this knowledge and to pass it on to others in the company, they used a flow diagram to summarize the design procedure they had developed.

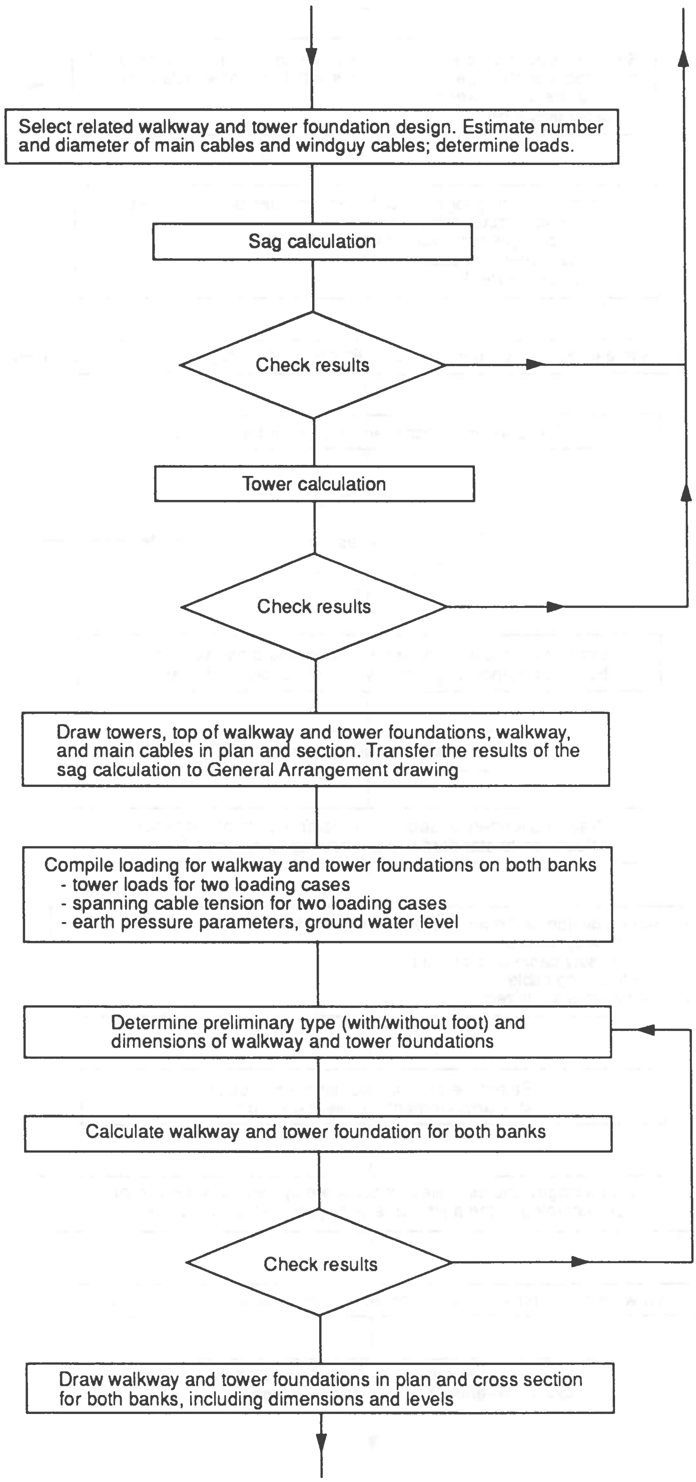
Design procedure for a reinforced concrete bridge

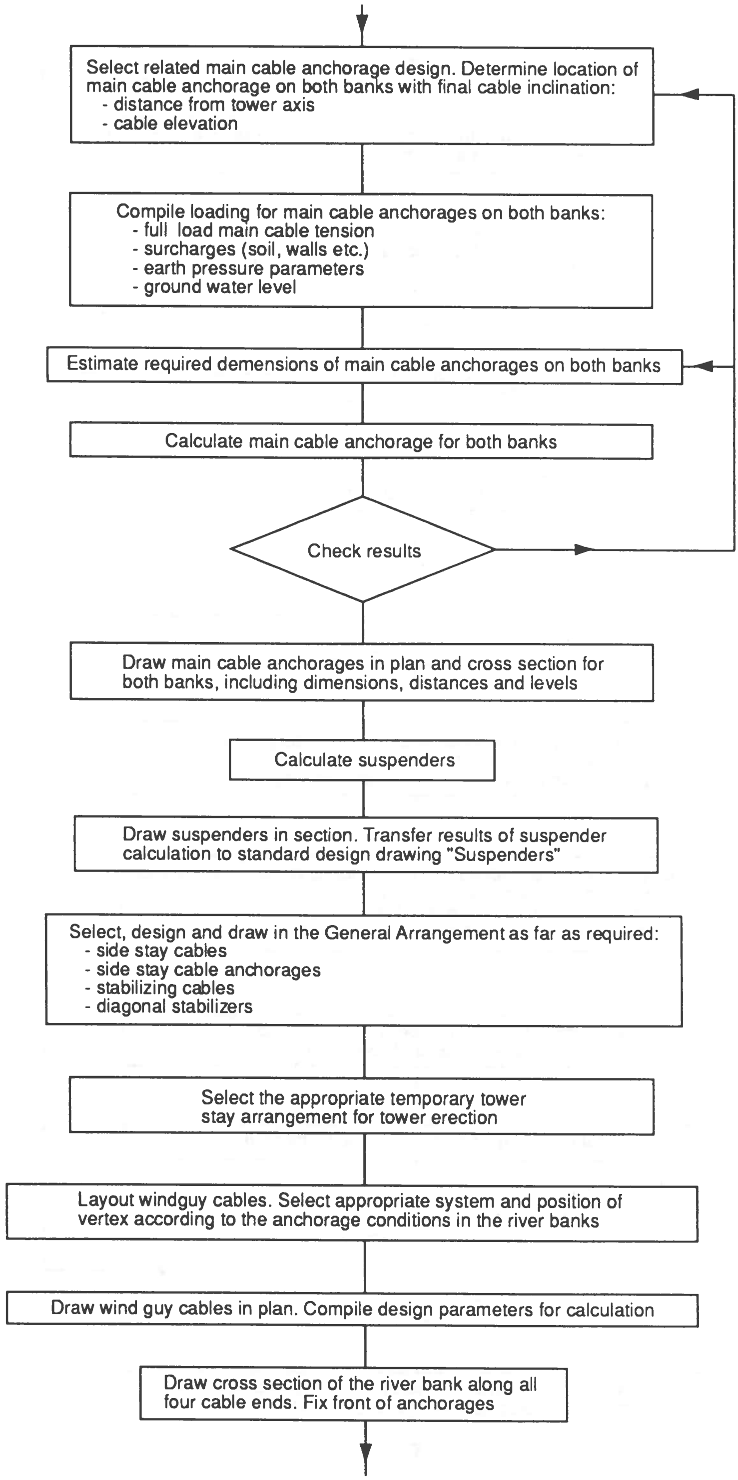


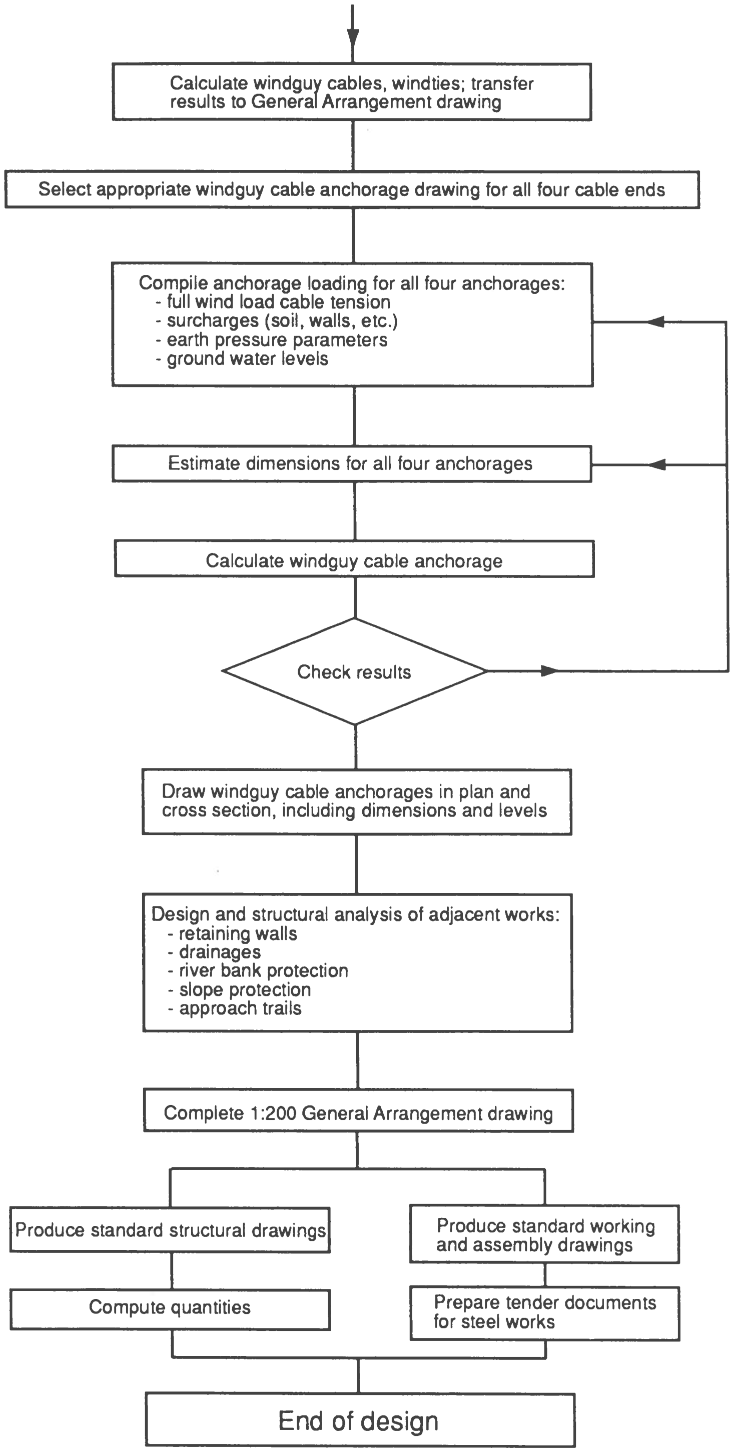
The second example is from a book which resulted from the unusual circumstances facing one engineering design community when it needed to communicate its skills and knowledge to another in a different culture [Grob *et al.* 1983]. A number of bridge engineers were engaged in a project with the Swiss Company for International Technical Cooperation and Development (ITECO) to help to provide a number of similar pedestrian suspension bridges across rivers in mountainous regions of Nepal. Rather than simply construct the bridges and withdraw, the Swiss engineers chose to use the project to train local people with technical knowledge how to undertake every aspect of the project from the decision to build a suspension bridge, through the surveying, design, contractual arrangements, and procurement, to the construction itself. The result is an unusually clear and complete summary of how to design and build a particular type of structure—just the sort of information which is lacking about so many projects in the past. The required information and the many activities, such as surveying, and the calculations involved in designing the various parts of the bridges, are related in a number of flow diagrams. The one reproduced below deals with the design stages of a wire cable suspension bridge, laterally braced by windguy cables.

Design procedure for a trail suspension bridge for remote areas




Select related walkway and tower foundation design. Estimate number and diameter of main cables and windguy cables; determine loads.
Sag calculation
Check results
Tower calculation
Check results
Draw towers, top of walkway and tower foundations, walkway, and main cables in plan and section. Transfer the results of the sag calculation to General Arrangement drawing
Compile loading for walkway and tower foundations on both banks
- tower loads for two loading cases
- spanning cable tension for two loading cases
- earth pressure parameters, ground water level
Determine preliminary type (with/without foot) and dimensions of walkway and tower foundations
Calculate walkway and tower foundation for both banks
Check results
Draw walkway and tower foundations in plan and cross section for both banks, including dimensions and levels


Select related main cable anchorage design. Determine location of main cable anchorage on both banks with final cable inclination:
- distance from tower axis
- cable elevation
Compile loading for main cable anchorages on both banks:
- full load main cable tension
- surcharges (soil, walls etc.)
- earth pressure parameters
- ground water level
Estimate required demensions of main cable anchorages on both banks
Calculate main cable anchorage for both banks
Check results
Draw main cable anchorages in plan and cross section for both banks, including dimensions, distances and levels
Calculate suspenders
Draw suspenders in section. Transfer results of suspender calculation to standard design drawing "Suspenders"
Select, design and draw in the General Arrangement as far as required:
- side stay cables
- side stay cable anchorages
- stabilizing cables
- diagonal stabilizers
Select the appropriate temporary tower stay arrangement for tower erection
Layout windguy cables. Select appropriate system and position of vertex according to the anchorage conditions in the river banks
Draw wind guy cables in plan. Compile design parameters for calculation
Draw cross section of the river bank along all four cable ends. Fix front of anchorages


Calculate windguy cables, windties; transfer results to General Arrangement drawing
Select appropriate windguy cable anchorage drawing for all four cable ends
Compile anchorage loading for all four anchorages:
- full wind load cable tension
- surcharges (soil, walls, etc.)
- earth pressure parameters
- ground water levels
Estimate dimensions for all four anchorages
Calculate windguy cable anchorage
Check results
Draw windguy cable anchorages in plan and cross section, including dimensions and levels
Design and structural analysis of adjacent works:
- retaining walls
- drainages
- river bank protection
- slope protection
- approach trails
Complete 1:200 General Arrangement drawing
Produce standard structural drawings
Produce standard working and assembly drawings
Compute quantities
Prepare tender documents for steel works
End of design

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Name index

Subject index

The cover shows a detail of the roof of Liverpool Street Station, London; Engineer — Edward Wilson; Contractor — Fairbairn Engineering Company; Completed in 1875.

A graduate of Downing College Cambridge in engineering and philosophy, **Dr Addis** gained his Ph.D. from the University of Reading in 1987. He has lectured there since 1985 in the Department of Construction Management.

ANALYTICAL MODELLING OF STRUCTURAL SYSTEMS

IAIN A. MACLEOD, Professor of Structural Engineering, University of Strathclyde

This book is probably the first of a new genre of texts in structural analysis. It considers material and element models, highlighting the validity of assumptions, and presents a discussion of structural systems models that will help designers to create good finite element models of structural systems and to test the validity of the solutions obtained.

EMBANKMENTS ON SOFT CLAYS

S. LEROUEIL, Associate Professor, Department of Civil Engineering, Laval University, Quebec, Canada; J-P. MAGNAN, Associate Professor, L'Ecole Nationale des Ponts et Chaussées and F. TAVENAS, Professor, Department of Civil Engineering, University Laval, Quebec, Canada

Translator: DAVID MUIR WOOD, Department of Engineering, University of Cambridge

This rational approach to the design of embankments on soft clays has been developed by the authors of this book by laboratory and field observations. It covers the fundamentals of soft clay behaviour, methods of geotechnical investigation, behaviour of clay foundations, design and construction methods, and general guidelines for design and construction.

GROUP THEORY AND G-VECTOR SPACES IN STRUCTURAL ANALYSIS

Vibration, Stability and Statics

GEORGE M. ZLOKOVIC, Professor of Structure Systems and Space Structures, Faculty of Architecture, University of Belgrade, Yugoslavia

This book, written by an international figure in structural engineering, explores group theory and G-vector spaces in the analysis of structures. On the basis of abstract group theory, symmetry groups, vector spaces and theory of group representations, the theoretical basis and algorithmic procedures in G-vector spaces, entitled the G-vector analysis, are formulated for elements with complex symmetry in general, and specifically for methods dealing with vibration, stability and statics of structures.

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